

THURSDAY, SEPTEMBER 21, 1893.

THE BRITISH ASSOCIATION.

THE Nottingham meeting of the British Association must be recorded as a useful and pleasant one. There has been no tremendous sensation, but on the other hand there has not been much dullness. The weather up to Tuesday was everything that could be desired, and the proceedings were wound up yesterday by an innovation in the shape of a special performance of *Pharaoh* by Mr. Wilson Barrett and his company, to which the members of the Association were invited by the local committee.

The formal business of the Association was commenced on the 13th by a meeting of the general committee. From the council's report we cull the following announcements. The following were elected corresponding members:—Dr. Svante Arrhenius, Stockholm; Prof. Marcel Bertrand, Paris; Prof. F. Elfving, Helsingfors; Prof. Léo Errera, Brussels; Prof. G. Fritsch, Berlin; Mr. D. C. Gilman, Baltimore; Dr. C. E. Guillaume, Sèvres; Prof. Rosenthal, Erlangen; Dr. Maurits Snellen, Utrecht.

The council had drawn the attention of the Local Government Board to the desirability of the publication of the "Report on the Examination into Deviations from the Normal amongst 50,000 Children in various Schools," which had been presented to that board by the British Medical Association, and of several departments to the anthropometric method for the measurement of criminals, which is successfully in operation in France, Austria, and other continental countries.

At the meeting in the evening, in the Albert Hall, the retiring president, Sir A. Geikie, vacating the chair, spoke as follows:—Ladies and gentlemen, the last duty which your president for the year has to perform is to vacate the chair and formally introduce the new president. Allow me to thank you for the high honour of occupying the chair of the British Association, and at the same time to express the satisfaction with which I learn that the affairs of the British Association are in as satisfactory a condition as that in which I found them. The introduction of my successor is only a matter of form. His name is familiar to all of you, and he is esteemed all over the world as one of the great leaders of biological science—a great leader as well as a great investigator. He will speak to you with the authority of an acknowledged master of science. I have, therefore, great pleasure in introducing my successor in the chair, the Oxford professor, Dr. Burdon-Sanderson.

The President then delivered the address which we printed last week. Dr. Burdon-Sanderson's reference to the importance of the endowment of research has called forth remarks in the Press which clearly indicate that the people of this country do not yet see that in the peaceful war among nations that nation will win which has all the resources of science most easily at its command; that these resources are as much the first line of defence as the British Navy in actual war is our first line; and that with regard to them we are getting relatively worse equipped each year in consequence of the care with which science is being fostered by foreign governments and neglected by our own. We are in the same position to day with regard to science as we were in the days of Queen Elizabeth with regard to the Navy.

The sectional meetings began next day. The addresses

of the various presidents have shown a high level of excellence. This week we give those delivered in Sections C, D, G and H.

On Monday another meeting of the general committee was held in the afternoon for the purpose of determining upon the place of meeting in 1895. A deputation attended from Toronto, and Prof. Mavor, speaking on behalf of a local provisional committee, explained the facilities which Toronto afforded for the holding of general and sectional meetings. He also dwelt on the industries of the neighbourhood, its agriculture, and objects of scientific interest in the vicinity. It was eventually agreed that the committee express their best thanks to the provisional committee of Toronto for the invitation, and provided that suitable arrangements could be made similar to those that were made for the Montreal visit the committee would be prepared to entertain the question of meeting at Toronto before many years had elapsed.

Deputations were then introduced from Bournemouth and Ipswich. It appeared from information given by Mr. Griffith, the secretary, that Bournemouth offered greater facilities than Ipswich in the way of rooms for the meetings of the Sections. On the question being put to the vote, there appeared 31 for Ipswich and 20 for Bournemouth. It was, therefore, decided that the meeting of 1895 should be held at Ipswich.

The Marquis of Salisbury was nominated president of the meeting next year at Oxford. It was pointed out that among his claims he has been Chancellor of the University of Oxford since 1880, that he would therefore represent both hosts and guests, that he is a distinguished statesman, a courteous gentleman, a member of the council of the Royal Society, and a true man of science.

A list of vice-presidents was agreed to, and the meeting at Oxford was fixed for August 8.

The business concluded with the reappointment of Sir Douglas Galton and Mr. Vernon Harcourt as general secretaries, and Mr. G. Griffith as assistant general secretary, and Prof. Rücker as general treasurer.

The list of awards arrived at yesterday was as follows:

Electrical Standards	£
Meteorological Photographs	25
Mathematical Tables	10
Solar Radiation	15
National Physical Laboratory	15
Wave-length Tables	5
Iron and Steel Analysis	10
Action of Light on Dyed Colours	15
Erratic Blocks	5
Fossil Phyllopora	15
Geological Photographs	5
Shell-bearing Deposits at Clava, &c.	10
Eurypterids of the Pentland Hills	20
Sections of Stonesfield Slate	5
Earth Tremors	25
Exploration of Calf Hole Cave	50
Naples Zoological Station	5
Plymouth Zoological Station	100
Zoology of Sandwich Islands	15
Zoology of Irish Sea	100
Structure of Mammalian Heart	40
Climatology of Tropical Africa	10
Observations in South Georgia	10
Exploration in Arabia	50
Economic Training	30
Anthropometric Statistics	10
Ethnography of United Kingdom	5
The Glastonbury Village	10
Anthropometry in Schools	40
Mental and Physical Condition of Children	5
Corresponding Societies	20
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SECTION C.

GEOLOGY.

OPENING ADDRESS BY J. J. H. TEALL, M.A., F.R.S.,
SEC. G.S., PRESIDENT OF THE SECTION.

IT is a striking and remarkable fact that, although enormous progress has been made in petrographical science during the last hundred years, there has been comparatively little advance so far as broad, general theories relating to the origin of rocks are concerned. In Hutton's "Theory of the Earth," the outlines of which were published in 1783, the following operations are clearly recognised:—The degradation of the earth's surface by aqueous and atmospheric agencies; the deposition of the *débris* beneath the waters of the ocean; the consolidation and metamorphosis of the sedimentary deposits by the internal heat and by the injection of molten mineral matter; the disturbance and upheaval of the oceanic deposits; and, lastly, the formation of rocks by the consolidation of molten material both at the surface and in the interior of the earth.

Hutton regarded these operations as efficient causes ordained for the purpose of producing an earth adapted to sustain animal and vegetable life. His writings are saturated with the teleological philosophy of the age to which they belong, and some of his arguments strike us, therefore, as strange and inconclusive; moreover, the imperfect state of the sciences of chemistry and physics occasionally led him into serious error. Notwithstanding these imperfections, we are compelled to admit, when viewing his work in the light of modern knowledge, that we can find the traces, and sometimes far more than the traces, of those broad general theories relating to dynamical geology which are current at the present day.

If Hutton had contented himself with proving the reality of the agencies to which reference has been made it is probable that his views would have been generally accepted. But he went much further than this, and boldly maintained that one or other of these agencies, or several combined, would account for all the phenomena with which the geologist has to deal. It was this that gave rise to the controversial fire which blazed up with such fury during the early years of this century, and whose dying embers have not yet been extinguished.

The views of Hutton were in strong contrast to those of Werner, the celebrated professor of mineralogy at Freiberg, to whom science owes a debt of gratitude as great as that due to the Scottish physician. The value of a man's work must not simply be judged by the truth of the theory which he holds. I consider that the Wernerian theory—by which I understand a reference to the early stages of planetary evolution for the purpose of explaining certain geological facts—has been on the wane from the time it was propounded down to the present day; but I claim to be second to none in my admiration for the knowledge, genius, and enthusiasm of the illustrious Saxon professor. The uniformitarian doctrines of Hutton gave a very decided character to the theoretical views of British geologists during the middle of the century, in consequence of the eloquent support of Lyell; but of late there has been a tendency to hark back to a modified form of Wernerism. This tendency can, I think, be largely traced to the recognition of evolution as a factor in biology and physical astronomy. The discoveries in these sciences may necessitate a modification of the views held by some of the extreme advocates of uniformitarianism. This admission, however, by no means carries with it the conclusion that the methods based on the doctrine of uniformitarianism must be discarded. If I read the history of geology aright, every important advance in the theoretical interpretation of observed facts relating to physical geology has been made by the application of these methods. It does not, of course, follow that the progress in the future will be exactly along the same lines as that in the past; but, if I am right in the opinion I have expressed, it is a strong reason for adhering to the old methods until they have been proved to be inapplicable to at least some of the facts with which the physical geologist has to deal. Let us consider for a moment whether the recognition of evolution as a factor in biology and physical astronomy gives an *à priori* probability to some form of Wernerism.

The period of time represented by our fossiliferous records is perhaps equivalent to that occupied by the evolution of the vertebrata, but all the great subdivisions of the invertebrata were living in the Cambrian period, and must have been differentiated in still earlier times. Is it not probable, therefore, that the fossiliferous records at present known represent a period in-

significant in comparison with that during which life has existed upon the earth? Again, is it not probable that the period during which life has existed is a still smaller fraction of that which has elapsed since the formation of the primitive crust? And if so, what *à priori* reason have we for believing that the rocks accessible to observation contain the records of the early stages of the planet's history? But the advocates of the diluted form of Wernerism which find expression in geological writings at the present day almost invariably refer to recent speculations in cosmical physics. The views of astronomers have always had a powerful influence on those of geologists. Hutton wrote at a time when the astronomical world had been profoundly affected by Lagrange's discovery, in 1776, of the periodicity of the secular changes in the forms of the planetary orbits. The doubts as to the stability of the solar system which the recognition of these changes had inspired were thus removed, and astronomers could then see in the physical system of the universe "no vestige of a beginning,—no prospect of an end." Now it is otherwise. Tidal friction and the dissipation of energy by the earth and by the sun are each referred to as fixing a limit to the existing conditions. I have not the knowledge necessary to enable me to discuss these questions, and I will therefore admit, for the sake of argument, that the phenomena referred to indicate the lines along which the physical evolution of our planet has taken place; but does it follow that geologists should desert a working hypothesis which has led to brilliant results in the past for one which has been tried again and again and always found wanting?

If there were absolute unanimity amongst mathematical physicists, it might be necessary for us to reconsider our position. This, however, is not the case. After referring to the argument from tidal friction, Prof. Darwin, in his address to the Mathematical and Physical Section for 1886, says:—"On the whole, then, I can neither feel the cogency of the argument from tidal friction itself, nor, accepting it, can I place any reliance on the limits which it assigns to geological history." In reviewing the argument from the secular cooling of the earth, he points out that the possibility of the generation of heat in the interior by tidal friction has been ignored, and that the thermal data on which the calculations are based are not sufficiently complete to remove all reasonable doubt. He regards the case depending on the secular cooling of the sun as the strongest; but it is evident that, in view of undreamt-of possibilities, he would not allow it to have much weight in the face of adverse geological evidence. In conclusion he says:—"Although speculations as to the future course of science are usually of little avail, yet it seems as likely that meteorology and geology will pass the word of command to cosmical physics as the converse. At present our knowledge of a definite limit to geological time has so little precision that we should do wrong to summarily reject any theories which appear to demand longer periods of time than those which now appear allowable. In each branch of science hypothesis forms the nucleus for the aggregation of observation, and as long as facts are assimilated and co-ordinated we ought to follow our theory." Now, my point is that the uniformitarian hypothesis, as applied to the rocks we can examine, has assimilated and co-ordinated so many facts in the past, and is assimilating and co-ordinating so many new discoveries, that we should continue to follow it, rather than plunge into the trackless waste of cosmogonical speculation in pursuit of what may after all prove to be a will-o'-the-wisp.

As an additional illustration of the want of agreement amongst mathematical physicists on questions relating to the earth, I may refer to certain papers by Mr. Chree.¹ This author maintains that the modern theory of elasticity points to the conclusion that if a spherical globe, composed of a nearly incompressible elastic solid of the size of the earth, were set rotating as the earth is rotating, it would take the form which the earth actually possesses. How is the question of the fixity of the earth's axis affected by Mr. Chree's researches, and by the recent observations which prove a simultaneous change of latitude, in opposite directions, in Europe and at Honolulu? If geological facts point to a shifting of the position of the axis, is there any dynamical reason why they should not receive due consideration? Geologists want as much freedom as possible. We do not object to any limitations which are necessary in the interest of science, and we cordially welcome, and as a matter

¹ C. Chree, "On Some Applications of Physics and Mathematics to Geology," *Phil. Mag.*, vol. xxxii. (1891), pp. 233, 342.

of fact are largely dependent upon, assistance from other departments of knowledge; but those who would help us should bear in mind that the problems we have still to solve are extremely difficult and complex, so that if certain avenues of thought are closed on insufficient grounds by arguments of the validity of which we are unable to judge, but which we are naturally disposed to take on trust, the difficulties of our task may be greatly augmented and the progress of science seriously retarded. So far as I can judge, there is no *a priori* reason why we should believe that any of the rocks we now see were formed during the earlier stages of planetary evolution. We are free to examine them in our own way, and to draw on the bank of time to any extent that may seem necessary.

For some years past the greater part of my time has been devoted to a study of the composition and structure of rocks, and it has occurred to me that I might, on the present occasion, give expression to my views on the question as to whether the present position of petrographical science necessitates any important modification in the theoretical views introduced by the uniformitarian geologists. Must we supplement the ideas of Hutton and Lyell by any reference to primordial conditions when we endeavour to realise the manner in which the rocks we can see and handle were produced? The question I propose to consider is not whether some of these rocks *may* have been formed under physical conditions different from those which now exist—life is too short to make a discussion of geological possibilities a profitable pursuit—but whether the present state of petrographical science renders uniformitarianism untenable as a working hypothesis; and, if so, to what extent. There is nothing original in what I am about to lay before you. All that I propose to do is to select from the numerous facts and more or less conflicting views, bearing on the question I have stated, a few of those which appear to me to be of considerable importance.

The sedimentary rocks contain the history of life upon the earth, and on this account, as well as on account of their extensive development at the surface, they have necessarily received an amount of attention which is out of all proportion to their importance as constituent portions of the planet. They are, after all, only skin deep. If they were totally removed from our globe its importance as a member of the solar system would not be appreciably diminished. The general laws governing the formation and deposition of these sediments have been fairly well understood for a long time. Hutton, as we have already seen, clearly realised that the land is always wasting away, and that the materials are accumulating on the beds of rivers, lakes, and seas. The chemical effects of denudation are mainly seen in the breaking up of certain silicates and the separation of their constituents into those which are soluble and those which are insoluble under surface conditions. The mechanical effects are seen in the disintegration of rocks, and this may, under certain circumstances, take place without the decomposition of their component minerals.¹ Quartz and the aluminous silicates, which enter largely into the composition of shales and clays, are two of the most important insoluble constituents. It must be remembered, however, that felspars often possess considerable powers of resistance, and rocks which contain them may be broken up without complete or anything like complete decomposition of these minerals. Orthoclase, microcline, and oligoclase are the varieties which most successfully resist decomposition; and, as a natural consequence, occur most abundantly in sedimentary deposits. It is commonly stated that when felspars are attacked the general effect is to reduce them to a fine powder, composed of a hydrated silicate of alumina, and to remove the alkalis, lime, and a portion of the silica. But, as Dr. Sterry Hunt has so frequently urged, the removal of alkalis is imperfect, for they are almost invariably present in argillaceous deposits. Three, four, and even five per cent., consisting mainly of potash, may frequently be found. This alkali appears to be present in micaceous minerals, which are often produced, as very minute scales, during the decomposition of felspars. White mica, whether formed in this way or as a product of igneous or metamorphic action, possesses great powers of resistance to the ordinary surface agencies of decomposition, and so may be used over and over again in the making of sedimentary deposits. Brown mica is also frequently separated from granite and other rocks, and deposited as a constituent of sediments; but it is far more liable

to decomposition than the common white varieties, and its geological life is, therefore, comparatively short.¹ Small crystals and grains of zircon, rutile, ilmenite, cyanite, and tourmaline are nearly indestructible, and occur as accessory constituents in the finer-grained sand-tones.² Garnet and staurolite also possess considerable powers of resistance, and are not unfrequently present in the same deposits. If we except the last two minerals and a few others, such as epidote, the silicates containing lime, iron, and magnesia are, as a rule, decomposed by surface agencies and the bases removed in solution; augite, enstatite, hornblende, and lime-felspars are extremely rare as constituents of ordinary sediments.

The insoluble constituents resulting from the waste of land surfaces are deposited as gravel, sand, and mud; the soluble constituents become separated as solid bodies by evaporation of the water in inland seas and lagoons, by chemical action, and by organic life. They are deposited as carbonates, sulphates, chlorides, and sometimes, as in the case of iron and manganese, as oxides. The soluble silica may be deposited in the opaline condition by the action of sponges, radiolaria, and diatoms, or as sinter.

The question that we have now to consider is whether there is any marked difference between ancient and modern sediments. One of the oldest deposits in the British Isles is the Torridon sandstone of the north-west of Scotland. The recent discovery of *Olenellus* high up in the stratified rocks which unconformably overlie this deposit has placed its pre-Cambrian age beyond all doubt. Now this formation is mainly composed of quartz and felspar, at least in its upper part, and the latter mineral is both abundant and very slightly altered. One is naturally tempted, at first sight, to associate the freshness of the felspar with the great age of the rock—to assume either that the sand was formed at a time when the chemical agents of decomposition did not act with the same force as now, or that they had not been in operation for a sufficient length of time to eliminate the felspar. A pure quartzose sand is probably never formed by the direct denudation of a granitic or gneissic area. The coarser sediments thus produced contain in most, if not in all, cases a considerable amount of felspar. But felspar is more liable to decomposition by percolating waters when it occurs as a constituent of grit than when present in the parent rock. Silica may thus be liberated in a soluble form, and subsequently deposited on the grains of quartz so as to give rise to secondary crystalline faces, and kaolin may be produced as beautiful six-sided tablets in the interstices of the grit. When the grit is in its turn denuded the felspar is still further reduced in amount, and a purer quartz-sand is formed. As the coarser detrital material is used over and over again, thus measuring different periods of time like the sand in an hour-glass, the felspar and other decomposable minerals are gradually eliminated. The occurrence of a large amount of fresh felspar in the Torridon sandstone might, I say, at first sight be thought to be due to the great age of the rock. Any tendency to accept a view of this kind is, however, at once checked when attention is paid to the pebbles in the coarser conglomeratic beds of the same deposit. These consist largely of quartzite—a rock formed by the consolidation of as pure a quartz-sand as any known to exist in the later formations. We are therefore led to the conclusion that the special features of the Torridon sandstone are not a function of time, but of the local conditions under which the rock was produced.

A similar conclusion may be reached by considering other types of sediment. When the stratified rocks of the different geological periods represented in any limited area are compared with each other certain marked differences may be observed, but the different types formed in any one area at different times can often be paralleled with the different types formed in different areas at the same time, and also with those now forming beneath the waters of rivers, lakes, and seas. Deep sea, shallow water, littoral and terrestrial deposits can be recognised in the formations belonging to many geological periods, from the most ancient to the most recent; and there is no evidence that any of our sedimentary rocks carry us back to a time when the physical conditions of the planet were materially different

¹ "Notes on the Probable Origin of Some Slates," by W. Maynard Hutchings, *Geol. Mag.*, 1890, p. 264.

² "Ueber das Vorkommen mikroskopischer Zirkone und Titan-Mineralien," von Dr. Hans Thürach, *Verhandl. d. phys.-med. Gesellschaft zu Würzburg*, N.F. xviii. "On Zircon and other Minerals contained in Sand," Allan B. Dick, *NATURE*, vol. xxxvi. (1887), p. 91. See also "Mem. Geol. Survey," *Geology of London*, vol. i. p. 523.

¹ J. W. Judd, "Deposits of the Nile Delta," *Proc. Royal Soc.*, vol. xxxix. (1886), p. 213.

from those which now exist. After reviewing all the evidence at my disposal, I must, however, admit that the coarser as well as the finer deposits of the earlier periods appear to be more complex in composition than those of the later. The grits of the Palæozoic formations, taken as a whole, contain more felspar than the sandstones of the Mesozoic and Tertiary formations, and the slates and shales of the former contain more alkalis than the clays of the latter. This statement will hold good for the British Isles, even when allowance is made for the enormous amount of volcanic material amongst the older rocks—a phenomenon which I hold to be of purely local significance—but I strongly suspect that it will not be found to apply universally. In any case, it is not of much importance from our present point of view. All geologists will admit that denudation and deposition were taking place in pre-Cambrian times, under chemical and physical conditions very similar to, if not identical with, those of the present day.

There is, however, one general consideration of more serious import. Additions to the total amount of detrital material are now being made by the decomposition of igneous rocks, and there is no doubt that this has been going on during the whole period of time represented by our stratified deposits. It follows, therefore, as a necessary consequence that strict uniformitarianism is untenable, unless we suppose that igneous magmas are formed by the melting of sediments.

So far we have been dealing with the characters of sedimentary rocks as seen in hand-specimens rather than with those which depend on their distribution over large areas. Thanks to Delesse ("Lithologie du Fond des Mers," Paris, 1871) and the officers of the *Challenger* Expedition ("Report on Deep-sea Deposits," 1891), an attempt has now been made to construct maps on which the distribution of the sediments in course of formation at the present time is laid down. It is impossible to exaggerate the importance of such maps from a geological point of view, for on the facts which they express rests the correct interpretation of our stratigraphical records. Imperfect as is our knowledge of the sea-beds of former geological periods, it is in many respects more complete than that of the sea-beds of the present day. The former we can often examine at our leisure, and follow from point to point in innumerable exposures; the latter are known only from a few soundings, often taken at great distances apart.¹ An examination of such imperfect maps as we have raises many questions of great interest and importance, to one of which I wish to direct special attention—not because it is new, but because it is often overlooked. The boundary lines separating the distinct types of deposit on these maps are not, of course, chronological lines. They do not separate sediments produced at different times, but different sediments simultaneously forming in different places. Now, the lines on our geological maps are usually drawn by tracing the boundary between two distinct lithological types, and, as a natural consequence, such lines will not always be chronological lines. It is only when the existing outcrop runs parallel with the margin of the original area of deposit that this is the fact. Consider the case of a subsiding area—or, to avoid theory, let us say an area in which the water-level rises relatively to the land—and, for the sake of illustration, let us suppose that the boundary separating the districts over which sand and mud are accumulating remains parallel to the old coast-line during the period of deposition. This line will follow the retreating coast, so that if, after the consolidation, emergence, and denudation of the deposits, the outcrop happens to be oblique to the old shore, then the line on the geological map separating clay and sand will not be of chronological value. That portion of it which lies nearer to the position of the vanished land will represent a later period than that which lies further away. If such organisms as ammonites leave their remains in the different deposits, and thus define different chronological horizons with approximate accuracy, the imperfection of the lithological boundary as a chronological horizon will become manifest. It is not that the geological map is wrong. Such maps have necessarily to be constructed with reference to economic considerations, and from this point of view the lithological boundaries are of paramount importance. They are, moreover, in many cases the only boundaries that can be actually traced.²

¹ Suess, *Das Antlitz der Erde*, B.I. II., s. 267.

² See S. S. Buckman, "On the Corteswold, M. Jford, and Yewill Sands," *Quart. Journ. Geol. Soc.*, v. l. xlv. (1889), p. 440; and the same author, "On the So-called Upper Line Clay of Down Cliffs," *Quart. Journ. Geol. Soc.*, vol. xlv. (1890), p. 518. Also J. Starkie Gardner, "On the Relative Ages of the American and the English Cretaceous and Eocene Series," *Geol. Mag.* (1884), p. 492.

The geological millennium will be near at hand when we can construct maps which shall represent the distribution of the different varieties of sediment for each of the different geological periods. All we can say at present is that increase of knowledge in this direction tends greatly to strengthen the uniformitarian hypothesis. We can see, for example, that during Triassic times marine conditions prevailed over a large part of what is now the great mountain-belt of the Euro-Asiatic continent, whilst littoral and terrestrial conditions existed in the north of Europe; and we can catch glimpses of the onward sweep of the sedimentary zones during the great Cretaceous transgression, culminating in the widespread deep-sea conditions under which the chalk was deposited.

We turn now to the igneous rocks. It is no part of my purpose to treat in detail of the growth of knowledge from an historical point of view, and to attempt to allot to each observer the credit due to him; but there is one name that I desire to mention in this connection, because it is that of a man who clearly proved the essential identity of ancient and modern volcanic rocks by the application of precise petrographical methods at a time when there was a very general belief that the Tertiary and pre-Tertiary rocks were radically distinct. I need hardly say that I refer to Mr. Samuel Allport.³ He wrote at a time when observers in this country had to prepare their own sections, and those who, like myself, have had the privilege of examining many of his slides scarcely know which to admire most—the skill and patience of which they are the evidence, or the conciseness and accuracy of his petrographical descriptions. His papers do not occupy a large number of pages, but they are based on an amount of observation which is truly surprising. The general conclusions at which he arrived as to the essential identity of ancient and modern igneous rocks are expressed with the utmost confidence, and one feels, after going over his material, that this confidence was thoroughly justified. It is curious now to note that the one British champion of the distinctness of the Tertiary and pre-Tertiary rocks pointed to the difference between the Antrim and Limerick traps. These traps differ in exactly the same way as do the corresponding Tertiary and pre-Tertiary continental rocks, with this important difference. On the Continent the ophiitic structure is characteristic of the pre-Tertiary rocks, whereas in the north of Ireland it is a marked feature of those of Tertiary age. We see, therefore, that the arguments for the distinctness of the two sets of rocks derived from the two areas, based in both cases on perfectly accurate observations, neutralise each other, and the case hopelessly breaks down as regards the basalts and dolerites.

In this country it is now generally recognised that, when allowance is made for alterations which are necessarily more marked in the earlier than in the later rocks, there is no important difference either in structure or composition between the rhyolites, andesites, and basalts of the Palæozoic and Tertiary periods. But identity of structure and composition may in this case be taken to imply identity as to the physical conditions under which the rocks were produced. We are thus led to picture in our minds long lines of volcanoes fringing the borders of Palæozoic continents and rising as islands in the Palæozoic seas. Then, as now, there issued from the craters of these volcanoes enormous masses of fragmental material, a large portion of which was blown to dust by the explosive escape of steam and other gases from the midst of molten rock; and then, as now, there issued from fissures on their flanks vast masses of lava which consolidated as rhyolite, andesite, and basalt. We may sum up the case as regards the volcanic rocks by saying that, so long as observations are confined to a limited area, doubts may arise as to the truth of the uniformitarian view, but these doubts gradually fade away as the area of observation is extended. There are still some outstanding difficulties, such as the apparent absence of leucite lavas amongst the Palæozoic formations; but as many similar difficulties have been overcome in the past, it is improbable that those which remain are of a very formidable character.

So far we have been referring to rocks formed at the surface of the earth under conditions similar to those now in operation. But there are others, such as granite, gneiss, and mica schist,

³ Theodor Fuchs, "Welche Ablagerungen haben wir als Tiefseebildungen zu betrachten?" *Neues Jahrbuch f. Mineral. &c. Beilage*, Band II., p. 437.

⁴ "Tertiary and Palæozoic Trap-rocks," *Geol. Mag.* (1873), p. 196. "British Carboniferous Dolerites," *Quart. Journ. Geol. Soc.*, vol. xxx. (1874), p. 529. "Ancient Devitrified Pitch-tones," *Geol. Mag.*, vol. xxxiii (1877), p. 449.

which are obviously unlike any of the products of surface agencies. If these rocks are forming now, it must be beneath the surface. This point was clearly realised by Hutton. Granite was proved by him to be an igneous rock of subterranean origin. His conclusions as to the formation of the schists are expressed in a passage so remarkable when viewed in connection with what I regard as the tendency of modern research, that I make no apology for quoting it at length. "If, in examining our land, we shall find a mass of matter which had been evidently formed originally in the ordinary manner of stratification, but which is now extremely distorted in its structure, and displaced in its position—which is also extremely consolidated in its mass, and variously changed in its composition—which therefore has the marks of its original or marine composition extremely obliterated, and many subsequent veins of melted mineral matter interjected; we should then have reason to suppose that here were masses of matter which, though not different in their origin from those that are gradually deposited at the bottom of the ocean, have been more acted upon by subterranean heat and the expanding power, that is to say, have been changed in a greater degree by the operations of the mineral region. If this conclusion shall be thought reasonable, then here is an explanation of all the peculiar appearances of the Alpine schistus masses of our land, those parts which have been erroneously considered as primitive in the constitution of the earth ("Theory of the Earth," vol. i. p. 375). Surely it is not claiming too much for our author to say that we have there, sketched in broad outline, the theories of thermal and dynamic metamorphism which are attracting so much attention at the present day.

The hypogene origin of the normal plutonic rocks and their formation at different periods, even as late as the Tertiary, are facts which are now so generally recognised that we may leave these rocks without further comment and pass on to the consideration of the crystalline schists.

Everyone knows that the statement, "He who runs may read," is untrue when the stratigraphical interpretation of an intensely folded and faulted district is concerned. The complexity produced by the earth-movements in such regions can only be unravelled by detailed work after definite palæontological and lithological horizons have been established. But if the statement be untrue when applied to districts composed of ordinary stratified rocks, still less can it be true of regions of crystalline schist where the movements have often been much more intense; where the original characters of the rocks have been profoundly modified; and where all distinct traces of fossils have in most cases been obliterated. If detailed work like that of Prof. Lapworth at Dobb's Linn was required to solve the stratigraphical difficulties of the Southern Uplands, is it not probable that even more detailed work will be required to solve the structural problems of such a district as the Highlands of Scotland, where the earth-stresses, though somewhat similar, have operated with greater intensity, and where the injection of molten mineral matter has taken place more than once both on a large and on a small scale? With these few general remarks by way of introduction, I will now call attention to what appear to me to be the most promising lines of investigation in this department of geology.

The crystalline schists certainly do not form a natural group. Some are undoubtedly plutonic igneous rocks showing original fluxion; others are igneous rocks which have been deformed by earth-stresses subsequent to consolidation; others, again, are sedimentary rocks metamorphosed by dynamic and thermal agencies, and more or less injected with "molten mineral matter"; and lastly, some cannot be classified with certainty under any of these heads. So much being granted, it is obvious that we must deal with this petrographical complex by separating from it those rocks about the origin of which there can be no reasonable doubt. Until this separation has been effected, it is quite impossible to discuss with profit the question as to whether any portions of the primitive crust remain. In order to carry out this work it is necessary to establish some criterion by which the rocks of igneous may be separated from those of sedimentary origin. Such a criterion may, I think, be found, at any rate in many cases, by combining chemical with field evidence.¹ If associated rocks possess the composition of grits, sandstones, shales and limestones, and contain also traces of stratification, it seems perfectly justifiable to conclude that they

must have been originally formed by processes of denudation and deposition. That we have such rocks in the Alps and in the Central Highlands of Scotland, to mention only two localities, will be admitted by all who are familiar with those regions. Again, if the associated rocks possess the composition of igneous products, it seems equally reasonable to conclude that they are of igneous origin. Such a series we find in the North-West of Scotland, in the Malvern Hills, and at the Lizard. In applying the test of chemical composition it is very necessary to remember that it must be based, not on a comparison of individual specimens, but of groups of specimens. A granite and an arkose, a granitic gneiss and a gneiss formed by the metamorphosis of a grit, may agree in chemical and even in mineralogical composition. The chemical test would therefore utterly fail if employed for the purpose of discriminating between these rocks. But when we introduce the principle of paragenesis it enables us in many cases to distinguish between them. The granitic gneiss will be associated with rocks having the composition of diorites, gabbros, and peridotites; the sedimentary gneiss with rocks answering to sandstones, shales, and limestones. Apply this test to the gneisses of Scotland, and I believe it will be found in many cases to furnish a solution of the problem. Caution, however, is necessary; for crystal-building and the formation of segregation veins and patches in the sedimentary schists clearly prove that a migration of constituents takes place under certain circumstances.

Recent work on the gneisses and schists of igneous composition has shown that the parallel structure, by no means invariably present, is sometimes the result of fluxion during the final stages of consolidation, and sometimes due to the plastic deformation of solid rocks. When compared with masses of ordinary plutonic rock, the principal points of difference, apart from those due to secondary dynamic causes, depend on what may be called their extreme petrographical differentiation. Indications of differentiation may, however, be seen in the contemporaneous veins and basic patches so common in ordinary eruptive bosses, but they are never so marked as in gneissic regions, like those of the North-West of Scotland, where specimens answering in composition to granites, diorites, and even peridotites, may be collected repeatedly in very limited areas. The nearest approach to the conditions of gneissose regions is to be found in connected masses of diverse plutonic rocks, such as those which are sometimes found on the borders of great granitic intrusions.

The tectonic relations of those gneisses which resemble igneous rocks in composition fully bear out the plutonic theory as to their origin. Thus, the intrusive character of granitic gneiss in a portion of the Himalayas has been demonstrated by General McMahon.¹ The protogine of Mont Blanc has been investigated by M. Lévy² with the same result. Most significant of all are the discoveries in the vast Archaean region of Canada. Professor Lawson³ has shown that immense areas of the so-called Laurentian gneiss in the district north west of Lake Superior are intrusive in the surrounding rocks, and therefore newer, not older, than these. Professor Adams⁴ has quite recently established a similar fact as regards the anorthositic rocks—the so-called Norian—of the Saguenay River and other districts lying near the eastern margin of the "Canadian shield." Now that the intrusive character of so many gneisses is being recognised, one wonders where the tide of discovery will stop. How long will it be before the existence of gneisses of Tertiary age will be generally admitted? At any rate, the discoveries of recent years have compelled the followers of Wernerian methods to evacuate large slices of territory.

Turning now to the gneisses and schists which resemble sedimentary rocks in composition, we note that the parallel structure may be due to original stratification, to subsequent deformation, or to both of these agencies combined. It must also be remembered that they have often been injected with igneous material, as Hutton pointed out. Where this has followed parallel planes of weakness, we have a banding due to alternations of igneous and sedimentary material. This injection

¹ H. Rosenbusch, "Zur Auffassung der chemischen Natur des Grundgebirges," *Min. und petro. Mitth.*, xii. (1891), p. 49.

¹ "The Geology of Dalhousie," *Records of Geol. Survey of India*, vol. xv part 1 (1882), p. 34. See also vol. xvi part 3 (1883), p. 129.

² "Les Roches Crystallines et Eruptives des Environs du Mont-Blanc," *Bull. des Services de la Carte Géologique de la France*, No. 9 (1890).

³ "On the Geology of the Rainy Lake Region," *Annual Report Geol. Survey of Canada* for 1887.

⁴ "Ueber das Norian oder Ober-Laurentian von Canada," *Neues Jahrbuch. Mineralogie, &c.*, Beilage, Band viii. p. 419.

lit par lit has been shown by M. Lévy to be a potent cause in the formation of certain banded gneisses.

Will the various agencies to which reference has been made explain all the phenomena of the crystalline schists and gneisses? I do not think that the present state of our knowledge justifies us in answering this question in the affirmative. Those who are working on these rocks frequently have brought under their notice specimens about the origin of which they are not able to speak with any degree of confidence. Sometimes a flood of light is suddenly thrown on a group of doubtful rocks by the recognition of a character which gives unmistakable indications of their mode of origin. Thus, some of the fine-grained quartz-felspathic rocks associated with the crystalline schists of the Central Highlands are proved to have been originally sands like those of Hampstead Heath by the presence in them of narrow bands rich in zircon, rutile, and the other heavy minerals which are so constantly present in the finer-grained arenaceous deposits of all ages. Such pleasant surprises as the recognition of a character like this increase our confidence in the theory which endeavours to explain the past by reference to the present, and refuses to admit the necessity of believing in the existence of rocks formed under physical conditions different from those which now prevail simply because there are some whose origin is still involved in mystery.

A crystalline schist has been aptly compared to a palimpsest. Historical records of priceless value have often been obscured by the superposition of later writings; so it is with the records of the rocks. In the case of the schists, the original characters have been so modified by folding, faulting, deformation, crystallisation, and segregation that they have often become unrecognisable. But when the associated rocks have the composition of sediments we need have no hesitation in attributing the banded structure in some way to stratification, provided we clearly recognise that the order of succession and the relative thicknesses of the original beds cannot be ascertained by applying the principles which are valid in comparatively undisturbed regions.

In studying the crystalline schist: nothing, perhaps, strikes one more forcibly than the evidence of crystal-building in solid rocks. Chistolite, staurolite, andalusite, garnet, albite, corundum, micas of various kinds, and many other minerals have clearly been developed without anything like fusion having taken place. Traces of previous movements may not unfrequently be found in the arrangement of the inclusions, while the minerals themselves show no signs of deformation. Facts of this kind, when they occur, clearly indicate that the crystallisation was subsequent to the mechanical action. Nevertheless, it is probable that both phenomena were closely related, though not in all cases as cause and effect. The intrusion of large masses of plutonic rock often marks the close of a period of folding. This is well illustrated by the relation of granite to the surrounding rocks in the Lake District, the Southern Uplands of Scotland, and the West of England. Those of the two first-mentioned localities are post-Silurian and pre-Carboniferous, those of the last-mentioned locality are post-Carboniferous and pre-Permian; one set followed the Caledonian¹ folding, the other set followed the Hercynian folding. That the intrusion of these granites was subsequent to the main movements which produced the folding and cleavage is proved by the fact that the mechanical structures may often be recognised in the crystalline contact-rocks, although the individual minerals have not been strained or broken. In many other respects the rocks produced by so-called contact-metamorphism resemble those found in certain areas of crystalline schist. Many of the most characteristic minerals are common to the two sets of rocks, and so also are many structures. The cipolins and associated rocks of schistose regions have many points of resemblance to the crystalline limestones and "kalksilicathornfels" produced by contact-metamorphism.²

These facts make it highly probable that, by studying the metamorphic action surrounding plutonic masses, we may gain an insight into the causes which have produced the crystalline schists of sedimentary origin; just as, by studying the intrusive masses themselves and noting the tendency to petrographical differentiation, especially at the margins, we may gain an insight into the causes which have produced the gneisses of igneous

origin.¹ In the districts to which reference has been made the igneous material came from below into a region where the rocks had been rendered tolerably rigid. Differential movement was not taking place in these rocks when the intrusion occurred. Consider what must happen if the folding stresses operate on the zone separating the sedimentary rocks from the underlying source of igneous material. Intrusion must then take place during interstitial movement, fluxion structures will be produced in the more or less differentiated igneous magmas, the sediments will be injected and impregnated with igneous material, and thermo-metamorphism will be produced on a regional scale. The origin of gneisses and schists, in my opinion, is to be sought for in a combination of the thermal and dynamic agencies which may be reasonably supposed to operate in the deeper zones of the earth's crust. If this view be correct it is not improbable that we may have crystalline schists and gneisses of post-Silurian age in the North-West of Europe formed during the Caledonian folding, others in Central Europe of post-Devonian age due to the Hercynian folding, and yet others in Southern Europe of post-Cretaceous age produced in connection with the Alpine folding.² But if the existence of such schists should ultimately be established it will still probably remain true that rocks of this character are in most cases of pre-Cambrian age. May not this be due to the fact, suggested by a consideration of the biological evidence, that the time covered by our fossiliferous records is but a small fraction of that during which the present physical conditions have remained practically constant?

The good old British ship "Uniformity," built by Hutton and refitted by Lyell, has won so many glorious victories in the past, and appears still to be in such excellent fighting trim, that I see no reason why she should haul down her colours either to "Catastrophe" or "Evolution." Instead, therefore, of acceding to the request to "hurry up" we make a demand for more time. The early stages of the planet's history may form a legitimate subject for the speculations of mathematical physicists, but there seems good reason to believe that they lie beyond the ken of those geologists who concern themselves only with the records of the rocks.

In this address I have ventured to express my views on certain disputed theoretical questions, and I must not conclude without a word of caution. The fact is, I attach very little importance to my own opinions, at least on doubtful questions connected with the origin of the crystalline schists; but, as you have done me the honour to accept me as your President, I thought you might like to know my present attitude of mind towards some of the unsolved problems of geology. There is still room for legitimate difference of opinion on many of the subjects to which I have referred. Meanwhile, we cannot do better than remember the words with which one of our great living masters recently concluded an article on a controversial subject: "Let us continue our work and remain friends."

SECTION D.

BIOLOGY.

OPENING ADDRESS BY REV. H. B. TRISTRAM, M.A., LL.D., D.D., F.R.S., PRESIDENT OF THE SECTION.

IT is difficult for the mind to grasp the advance in biological science (I use the term biology in its wide etymological, not its recently restricted sense) which has taken place since I first attended the meetings of the British Association, some forty years ago. In those days, the now familiar expressions of

¹ G. Barrow, "On an Intrusion of Muscovite-biotite-gneiss in the South Eastern Highlands of Scotland, &c." *Quart. Journ. Geol. Soc.*, vol. xlix. (1893), p. 330.

² Some geologists maintain that this is the case, others deny it. See H. Reusch, "Die fossilienführenden krystallinischen Schiefer von Bergen in Norwegen," Leipzig (1883) J. Lehmann, "Über die Entstehung der altkrystallinischen Schiefergesteine, mit besonderer Bezugnahme auf das sächsische Granulitgebirge, Erzgebirge, Fichtelgebirge, und bairisch-böhmische Grenzgebirge," Bonn, (1884): T. G. Bonney, several papers on the Alps, and especially "On the Crystalline Schists and their Relation to the Mesozoic Rocks of the Lepontine Alps," *Quart. Journ. Geol. Soc.*, vol. xlv. (1890), p. 185; A. Heim, contribution to the discussion on the last paper; C. W. Gümbel, "Geognostische Beschreibung des K. Bayerns" and "Grundzüge der Geologie," Kassel (1883-1892).

Although it is convenient to speak of the three types of folding which have so largely influenced the structure of the European continent as if each belonged to a definite period, it is important to remember that this is not strictly true. The movements were prolonged; they probably crept slowly over the surface of the lithosphere, as did the zones of sedimentation, so that those of the same type are not in all places strictly contemporaneous.

¹ This term is employed in the sense in which it is used by Suess and Bertrand.

² H. Rosenbusch, "Zur Auffassung des Grundgebirges," *Neues Jahrb. f. Miner.*, Bd. II. (1889) p. 8.

"natural selection," "isolation," "the struggle for existence," "the survival of the fittest," were unheard of and unknown, though many an observer was busied in culling the facts which were being poured into the lap of the philosopher who should mould the first great epoch in natural science since the days of Linnaeus.

It is to the importance and value of field observation that I would venture in the first place to direct your attention.

My predecessors in this chair have been, of recent years, distinguished men who have searched deeply into the abstrusest mysteries of physiology. Thither I do not presume to follow them. I rather come before you as a survivor of the old-world naturalist, as one whose researches have been, not in the laboratory or with the microscope, but on the wide desert, the mountain side, and the isles of the sea.

This year is the centenary of the death of Gilbert White, whom we may look upon as the father of field naturalists. It is true that Sir T. Browne, Willughby, and Ray had each, in the middle of the seventeenth century, committed various observations to print; but though Willughby, at least, recognised the importance of the soft parts in affording a key to classification, as well as the osteology, as may be seen from his observation of the peculiar formations, in the Divers (*Colymbidae*) of the tibia, with its prolonged procnemial process, of which he has given a figure, or his description of the elongation of the posterior branches of the woodpecker's tongue, as well as by his careful description of the intestines of all specimens which came under his notice in the flesh, none of these systematically noted the habits of birds, apart from an occasional mention of their nidification, and very rarely do they even describe the eggs. But White was the first observer to recognise how much may be learnt from the life habits of birds. He is generally content with recording his observations, leaving to others to speculate. Fond of Virgilian quotations (he was a fellow of Oriel of the last century), his quotations are often made with a view to prove the scrupulous accuracy of the Roman poet, as tested by his (White's) own observations.

In an age, incredulous as to that which appears to break the uniformity of nature, but quick to recognise all the phenomena of life, a contrast arises before the mind's eye between the abiding strength of the objective method, which brings Gilbert White in touch with the great writers whose works are for all time, and the transient feebleness of the modern introspective philosophies, vexed with the problems of psychology. The modern psychologist propounds his theory of man and the universe, and we read him, and go on our way, and straightway forget. Herodotus and Thucydides tell a plain tale in plain language, or the Curate of Selborne shows us the hawk on the wing, or the snake in the grass, as he saw them day by day, and, somehow, the simple story lives and moves him who reads it long after the subtleties of this or that philosophical theory have had their day and passed into the limbo of oblivion. But, invaluable as has been the example of Gilbert White in teaching us how to observe, his field was a very narrow one, circumscribed for the most part by the boundaries of a single parish, and on the subject of geographical distribution (as we know it now) he could contribute nothing, a subject on which even the best explorers of that day were strangely inobservant and inexact. A century and a half ago, it had not come to be recognised that distribution is, along, of course, with morphology and physiology, a most important factor in determining the facts of biology. It is difficult to estimate what might have been gained in the case of many species, now irreparably lost, had Forster and the other companions of Captain Cook, to say nothing of many previous voyagers, had the slightest conception of the importance of noting the exact locality of each specimen they collected. They seem scarcely to have recognised the specific distinctions of the characteristic genera of the Pacific Islands at all, or if they did, to have dismissed them with the remark, "On this island was found a flycatcher, a pigeon, or a parrot similar to those found in New Holland, but with white tail-feathers instead of black, an orange instead of a scarlet breast, or red shoulders instead of yellow." As we turn over the pages of Latham or Shaw, how often do we find for locality one of the islands of the South Sea, and, even where the locality is given, subsequent research has proved it erroneous, as though the specimens had been subsequently ticketed; Le Vaillant described many of his South African birds from memory. Thus Latham, after describing very accurately *Rhipidura flabellifera*, from the south island of New Zealand, remarks, apparently on

Forster's authority, that it is subject to variation; that in the island of Tanna another was met with, with a different tail, &c., and that there was another variety in the collection of Sir Joseph Banks. Endless perplexity has been caused by the *Psittacus pygmaeus* of Gmelin (of which Latham's type is at Vienna) being stated in the inventory as from Botany Bay, by Latham from Otaheite, and in his book as inhabiting several of the islands of the South Seas, and now it proves to be the female *Psittacus palmarum* from the New Hebrides. These are but samples of the confusion caused by the inaccuracies of the old voyagers. Had there been in the first crew who landed on the island of Bourbon, I will not say a naturalist, but even a simple-hearted Leguat, to tell the artless tale of what he saw, or had there been among the Portuguese discoverers of Mauritius one who could note and describe the habits of its birds with the accuracy with which a Poulton could record the ways and doings of our Lepidoptera, how vastly would our knowledge of a perished fauna have been enriched! It is only since we learned from Darwin and Wallace the power of isolation in the differentiation of species, that special attention has been paid to the peculiarities of insular forms. Here the field naturalist comes in as the helpful servant of the philosopher and the systematist, by illustrating the operation of isolation in the differentiation of species. I may take the typical examples of two groups of oceanic islands, differing as widely as possible in their position on the globe, the Sandwich Islands in the centre of the Pacific, thousands of miles from the nearest continent, and the Canaries, within sight of the African coast; but agreeing in this, that both are truly oceanic groups, of purely volcanic origin, the ocean depths close to the Canaries, and between the different islands, varying from 1500 to 2000 fathoms. In the one we may study the expiring relics of an avifauna completely differentiated by isolation; in the other we have the opportunity of tracing the incipient stages of the same process.

The Sandwich Islands have long been known as possessing an avifauna not surpassed in interesting peculiarity by that of New Zealand or Madagascar; in fact, it seems as though their vast distance from the continent had intensified the influences of isolation. There is scarcely a passerine bird in its indigenous fauna which can be referred to any genus known elsewhere. But, until the very recent researches of Mr. Scott Wilson, and the explorations of the Hon. W. Rothschild's collectors, it was not known that almost every island of the group possessed one or more representatives of each of these peculiar genera. Thus, every island which has been thoroughly explored, and in which any extent of the primeval forest remains, possesses, or has possessed, its own peculiar species of *Hemignathus*, *Himatione*, *Phaenotis*, *Acrulocercus*, *Loxops*, *Lrepanis*, as well as of the massive-beaked finches, which emulate the *Gospiza* of the Galapagos. Prof. Newton has shown that while the greater number of these are probably of American origin, yet the South Pacific has contributed its quota to this museum of ornithological rarities, which Mr. Clarke very justly proposes to make a distinct biological sub-region.

That each of the islands of this group, however small, should possess a flora specifically distinct, suggests thoughts of the vast periods occupied in their differentiation.

In the Canary Islands, either because they are geologically more recent, or because of their proximity to the African coast, which has facilitated frequent immigrations from the continent, the process of differentiation is only partially accomplished. Yet there is scarcely a resident species which is not more or less modified, and this modification is yet further advanced in the westernmost islands than in those nearest to Africa. In Fuerteventura and Lanzarote, waterless and treeless, there is little change, and the fauna is almost identical with that of the neighbouring Sahara. There is a whin-chat, *Pratincola dacotia*, discovered by my companion, Mr. Meade-Waldo, peculiar to Fuerteventura, which may possibly be found on the opposite coast, though it has not yet been met with by any collectors there. Now, our whin-chat is a common winter visitant all down the West African coast, and it seems probable that isolation has produced the very marked characters of the Canaries form, while the continental individuals have been restrained from variation by their frequent association with their migratory relations. A similar cause may explain why the blackbird, an extremely common resident in all the Canary Islands, has not been modified in the least, since many migratory individuals of the same species sojourn every winter in the islands. Or take

the blue titmouse. Our familiar resident is replaced along the coast of North Africa by a representative species, *Parus ultramarinus*, differentiated chiefly by a black instead of a blue cap, and a slate-colored instead of a green back. The titmouse of Lanzarote and Fuerteventura is barely separable from that of Algeria, but is much smaller and paler, probably owing to scarcity of food and a dry desert climate. Passing 100 miles further to sea, to Grand Canary, we find in the woods and forests a bird in all respects similar to the Algerian in colour and dimensions, with one exception—the greater wing coverts of the Algerian are tipped with white, forming a broad bar when the wing is closed. This, present in the Fuerteventura form, is represented in the Canarian by the faintest white tips, and in the birds from the next islands, Tenerife and Gomera, this is altogether absent. This form has been recognised as *Parus tenerife*. Proceeding to the north-west outermost island, Palma, we find a very distinct species, with different proportions, a longer tail, and white abdomen instead of yellow. In the Ultima Thule, Hierro, we find a second very distinct species, resembling that of Tenerife in the absence of the wing bar and in all other respects, except that the back is green like the European, instead of slate as in all the other species. Thus we find in this group a uniform gradation of variation as we proceed further from the cradle of the race.

A similar series of modifications may be traced in the chaffinch (*Fringilla*), which has been in like manner derived from the North African *F. spodiogena*, and in which the extreme variation is to be found in the westernmost islands of Palma and Hierro. The willow wren (*Phylloscopus trechilus*), extremely numerous and resident, has entirely changed its habits, though not its plumage, and I have felt justified in distinguishing it as *Ph. fortunatus*. In note and habits it is entirely different from our bird, and though it builds a domed nest it is always near the top of lofty trees, most frequently in palm-trees. The only external difference from our bird consists in its paler tarsi and more rounded wing, so that its power of flight is weaker, but, were it not for the marked difference in its habits and voice, I should have hesitated to differentiate it. In the kestrel and the great spotted woodpecker there are differences which suggest incipient species, while the forests of the wooded western islands yield two very peculiar pigeons, differing entirely from each other in their habits, both probably derived from our wood-pigeon, but even further removed from it than the *Columba tucos* of Madeira, and, by their dark chestnut coloration, suggesting that peculiar food, in this case the berries of the tree laurel, has its full share in the differentiation of isolated forms. If we remember the variability of the pigments in the food of birds, and the amount absorbed and transferred to the skin and plumage, the variability in the tints and patterns of many animals can be more readily understood.

One other bird deserves notice, the *Caccabis*, or red-legged partridge, for here, and here alone, we have chronological data. The Spaniards introduced *Caccabis rufa* into Canary, and *C. petrosa* into Tenerife and Gomera, and they have never spread from their respective localities. Now, both species, after a residence of only 400 years, have become distinctly modified. *C. rufa* was introduced into the Azores also, and changed exactly in the same manner, so much so that Mr. Godman, some years ago, would have described it as distinct, but that the only specimen he procured was in moult and mutilated, and his specimen proved identical with the Canarian bird. Besides minor differences, the back is one-fourth stouter and longer than in the European bird, and the tarsus very much stouter and longer, and the back is gray rather than russet. The gray back harmonises with the volcanic dark soil of the rocks of the Canaries, as the russet does with the clay of the plains of England and France. In the Canaries the bird lives under different conditions from those of Europe. It is on the mountain sides and among rocks that the stouter beak and stronger legs are indispensable to its vigorous existence. It is needless to go into the details of many other species. We have here the effect of changed conditions of life in 400 years. What may they not have been in 400 centuries? We have the result of peculiar food in the pigeons, and of isolation in all the cases I have mentioned. Such facts can only be supplied to the generaliser and the systematist through the accurate and minute observations of the field naturalist.

The character of the avifauna of the Comoro Islands, to take another insular group, seems to stand midway in the differentiating process between the Canaries and the Sandwich Islands.

From the researches of M. Humblot, worked out by MM. Milne-Edwards and Oustalet, we find that there are twenty-nine species acknowledged as peculiar; two species from South Africa and twenty-two from Madagascar in process of specification, called by M. Milne-Edwards secondary or derived species.

The little Christmas Island, an isolated rock 200 miles south of Java, only 12 miles in length, has been shown by Mr. Lister to produce distinct and peculiar forms of every class of life, vegetable and animal. Though the species are few in number, yet every mammal and land bird is endemic; but, as Darwin remarks, to ascertain whether a small isolated area, or a large open area like a continent, has been more favourable for the production of new organic forms, we ought to make the comparison between equal times, and this we are incapable of doing. My own attention was first directed to this subject when, in the year 1857–58, I spent many months in the Algerian Sahara, and noticed the remarkable variations in different groups, according to elevation from the sea, and the difference of soil and vegetation. The "Origin of Species" had not then appeared; but on my return my attention was called to the communication of Darwin and Wallace to the Linnean Society on the tendencies of species to form varieties, and on the perpetuation of varieties and species by means of natural selection. I then wrote (*Ibis* 1859, pp. 429–433): "It is hardly possible, I should think, to illustrate this theory better than by the larks and chats of North Africa. In all these, in the congeners of the wheatear, of the rock chat, of the crested lark, we trace gradual modifications of coloration and of anatomical structure, deflecting by very gentle gradations from the ordinary type, but, when we take the extremes, presenting the most marked differences. . . . In the desert, where neither trees, brushwood, nor even undulations of surface afford the slightest protection to an animal from its foes, a modification of colour, which shall be assimilated to that of the surrounding country, is absolutely necessary. Hence, without exception, the upper plumage of every bird—whether lark, chat, sylvan or land grouse—and also the fur of all the small mammals, and the skin of all the snakes and lizards, is of the uniform isabelline or sand-colour. It is very possible that some further purpose may be served by the prevailing colours, but this appears of itself a sufficient explanation. There are individual varieties of depth of hue among all creatures. In the struggle for life which we know to be going on among all species, a very slight change for the better, such as improved means of escape from its natural enemies (which would be the effect of an alteration from a conspicuous colour to one resembling the hue of the surrounding objects), would give the variety that possessed it a decided advantage over the typical or other forms of the species. . . . To apply the theory to the case of the Sahara. If the Algerian Desert were colonised by a few pairs of crested larks—putting aside the ascertained fact of the tendency of an arid, hot climate to bleach all dark colours—we know that the probability is that one or two pairs would be likely to be of a darker complexion than the others. These, and such of their offspring as most resembled them, would become more liable to capture by their natural enemies, hawks and carnivorous beasts. The lighter-coloured ones would enjoy more or less immunity from such attacks. Let this state of things continue for a few hundred years and the dark-coloured individuals would be exterminated, the light-coloured remain and inherit the land. This process, aided by the above-mentioned tendency of the climate to bleach the coloration still more, would in a few centuries produce the *Galerida abyssinica* as the typical form; and it must be noted that between it and the European *G. cristata* there is no distinction but that of colour.

"But when we turn to *Galerida isabellina*, *G. arenicola*, and *G. macrorhyncha*, we have differences, not only of colour, but of structure. These differences are most marked in the form of the bill. Now, to take the two former first, *G. arenicola* has a very long bill, *G. isabellina* a very short one; the former resorts exclusively to the deep, loose, sandy tracts, the latter haunts the hard and rocky districts. It is manifest that a bird whose food has to be sought for in deep sand derives a great advantage from any elongation, however slight, of its bill. The other, who feeds among stones and rocks, requires strength rather than length. We know that even in the type species the size of the bill varies in individuals—in the lark as well as in the snipe. Now, in the desert, the shorter-billed varieties would undergo comparative difficulty in finding food where it was not abundant, and consequently would not be in such vigorous condition as

their longer-billed relations. In the breeding season, therefore, they would have fewer eggs and a weaker progeny. Often, as we know, a weakly bird will abstain from matrimony altogether. The natural result of these causes would be that in course of time the longest-billed variety would steadily predominate over the shorter, and, in a few centuries, they would be the sole existing race; their shorter-billed fellows dying out until that race was extinct. The converse will still hold good of the stout-billed and weaker-billed varieties in a rocky district.

"Here are only two causes enumerated which might serve to create, as it were, a new species from an old one. Yet they are perfectly natural causes, and such as I think must have occurred, and are possibly occurring still. We know so very little of the causes which, in the majority of cases, make species rare or common that there may be hundreds of others at work, some even more powerful than these, which go to perpetuate and eliminate certain forms 'according to natural means of selection.'"

It would appear that those species in continental areas are equally liable to variation with those which are isolated in limited areas, yet that there are many counteracting influences which operate to check this tendency. It is often assumed, where we find closely allied species apparently inter-breeding at the centre of their area, that the blending of forms is caused by the two races commingling. Judging from insular experience I should be inclined to believe that the theory of inter-breeding is beginning at the wrong end, but rather that while the generalised forms remain in the centre of distribution, we find the more decidedly distinct species at the extremes of the range, caused not by inter-breeding, but by differentiation. To illustrate this by the group of the blue titmouse. We find in Central Russia, in the centre of distribution of the family, the most generalised form, *Parus pleskii*, partaking of the characters of the various species east, west, and south. In the north-east and north it becomes differentiated as *P. cyaneus*; to the south-west and south into *P. ceruleus* and its various sub-species, while a branch extending due east has assumed the form of *P. flavipectus*, bearing traces of affinity to its neighbour *P. cyaneus* in the north, which seems evidently to have been derived from it.

But the scope of field observation does not cease with geographical distribution and modification of form. The closet systematist is very apt to overlook or to take no count of habits, voice, modification, and other features of life which have an important bearing on the modification of species. To take one instance, the short-toed lark (*Calandrella brachydactyla*) is spread over the countries bordering on the Mediterranean; but, along with it, in Andalusia alone is found another species, *Cal. burtida*, of a rather darker colour, and with the secondaries generally somewhat shorter. Without further knowledge than that obtained from a comparison of skins, it might be put down as an accidental variety. But the field naturalist soon recognises it as a most distinct species. It has a different voice, a differently-shaped nest; and, while the common species breeds in the plains, this one always resorts to the hills. The Spanish shepherds on the spot recognise their distinctness, and have a name for each species. Take, again, the eastern form of the common song-thrush. The bird of North China, *Turdus auritus*, closely resembles our familiar species, but is slightly larger, and there is a minute difference in the wing formula. But the field naturalist has ascertained that it lays eggs like those of the missel-thrush, and it is the only species closely allied to our bird which does not lay eggs of a blue ground colour. The hedge accentor of Japan (*Accentor rubidus*) is distinguished from our most familiar friend, *Accentor modularis*, by delicate differences of hue. But, though in gait and manner it closely resembles it, I was surprised to find the Japanese bird strikingly distinct in habits and life, being found only in forest and brushwood several thousand feet above the sea. I met with it first at Chinsenzé—6000 feet—before the snow had left the ground, and in summer it goes higher still, but never descends to the cultivated land. If both species are derived, as seems probable, from *Accentor immaculatus* of the Himalayas, then the contrast in habits is easily explained. The lofty mountain ranges of Japan have enabled the settlers there to retain their original habits, for which our humbler elevations have afforded no scope.

On the solution of the problem of the migration of birds, the most remarkable of all the phenomena of animal life, much less aid has been contributed by the observations of field naturalists than might reasonably have been expected. The facts of migra-

tion have, of course, been recognised from the earliest times, and have afforded a theme for Hebrew and Greek poets 3000 years ago. Theories which would explain it are rife enough, but it is only of late years that any systematic effort has been made to classify and summarise the thousands of data and notes which are needed in order to draw any satisfactory conclusion. The observable facts may be classified as to their bearing on the whither, when, and how, of migration, and after this we may possibly arrive at a true answer to the Why? Observation has sufficiently answered the first question, Whither?

There are scarcely any feathered denizens of earth or sea to the summer and winter ranges of which we cannot now point. Of almost all the birds of the holo-arctic fauna, we have ascertained the breeding-places and the winter resorts. Now that the knot and the sanderling have been successfully pursued even to Grinnell Land, there remains but the curlew sandpiper (*Tringa subarquata*), of all the known European birds, whose breeding ground is a virgin soil, to be trodden, let us hope, in a successful exploration by Nansen, on one side or other of the North Pole. Equally clearly ascertained are the winter quarters of all the migrants. The most casual observer cannot fail to notice in any part of Africa, north or south, west coast or interior, the myriads of familiar species which winter there. As to the time of migration, the earliest notes of field naturalists have been the records of the dates of arrival of the feathered visitors. We possess them for some localities, as for Norfolk by the Marsham family, so far back as 1736. In recent years these observations have been carried out on a larger and more systematic scale by Middendorff, who, forty years ago, devoted himself to the study of the lines of migration in the Russian Empire, tracing what he called the *isopteres*, the lines of simultaneous arrival of particular species, and by Prof. Palmén, of Finland, who, twenty years later, pursued a similar course of investigation; and by Prof. Baird on the migration of North American birds; and subsequently by Severzoff as regards Central Asia, and Menzbier as regards Eastern Europe. As respects our own coasts, a vast mass of statistics has been collected by the labours of the Migration Committee appointed by the British Association in 1880, for which our thanks are due to the indefatigable zeal of Mr. John Cordeaux and his colleague Mr. John Harvie Brown, the originators of the scheme by which the lighthouses were for nine years used as posts of observation on migration. The reports of that committee are familiar to us, but the inferences are not yet worked out. I cannot but regret that the committee has been allowed to drop. Prof. W. W. Cooke has been carrying on similar observations in the Mississippi valley, and others, too numerous to mention, have done the same elsewhere. But, as Prof. Newton has truly said, all these efforts may be said to pale before the stupendous amount of information amassed during more than fifty years by the venerable Herr Gätke, of Heligoland, whose work we earnestly desire may soon appear in an English version.

We have, through the labours of the winter, I have named, and many others, arrived at a fair knowledge of the When? of migration. Of the How? we have ascertained a little, but very little. The lines of migration vary widely in different species, and in different longitudes. The theory of migration being directed towards the magnetic pole, first started by Middendorff, seems to be refuted by Baird, who has shown that in North America the theory will not hold. Yet, in some instances, there is evidently a converging tendency in northward migrations. The line, according to Middendorff, in Middle Siberia is due north, in Eastern Siberia south-east to north-west, and in Western Siberia from south-west to north-east. In European Russia Menzbier traces four northward routes: (1) A coast line coming up from Norway round the North Cape to Nova Zembla. (2) The Baltic line with bifurcation, one proceeding by the Gulf of Bothnia, and the other by the Gulf of Finland, which is afterwards again subdivided. (3) A Black Sea line, reaching nearly as far north as the valley of the Petchora; and (4) the Caspian line, passing up the Volga, and reaching as far east as the valley of the Obi by other anastomosing streams.

Palmén has endeavoured to trace the lines of migration on the return autumnal journey in the eastern hemisphere, and has arranged them in nine routes: (1) From Nova Zembla, round the West of Norway, to the British Isles. (2) From Spitzbergen, by Norway, to Britain, France, Portugal, and West Africa. (3) From North Russia, by the Gulf of Finland, Holstein, and Holland, and then bifurcating to the West-Coast of France on the one side, and on the other up the Rhine to Italy and North

Africa. (4a) Down the Volga by the Sea of Azof, Asia Minor, and Egypt, while the other portion (4b), trending east, passes by the Caspian and Tigris to the Persian Gulf. (5) By the Yenesei to Lake Baikal and Mongolia. (6) By the Lena on to the Amoor and Japan. (7) From East Siberia to the Corea and Japan. (8) Kamtschatka to Japan and the Chinese coast. (9) From Greenland, Iceland, and the Faroes, to Britain, where it joins line 2.

All courses of rivers of importance form minor routes, and consideration of these lines of migration might serve to explain the fact of North American stragglers, the waifs and strays which have fallen in with great flights of the regular migrants and been more frequently shot on the east coast of England and Scotland than on the west coast or in Ireland. They have not crossed the Atlantic, but have come from the far north, where a very slight deflection east or west might alter their whole course, and in that case they would naturally strike either Iceland or the west coast of Norway, and in either case would reach the east coast of Britain. But, if by storms, and the prevailing winds of the North Atlantic coming from the west, they had been driven out of their usual course, they would strike the coast of Norway, and so find their way hither in the company of their congeners.

As to the elevation at which migratory flights are carried on, Herr Gäcke, as well as many American observers, holds that it is generally far above our ken, at least in normal conditions of the atmosphere, and that the opportunities of observation, apart from seasons and unusual atmospheric disturbance, are confined chiefly to unsuccessful and abortive attempts. It is maintained that the height of flight is some 1500 to 15,000 feet, and if this be so, as there seems every reason to admit, the aid of land bridges and river valleys becomes of very slight importance. A trivial instance will illustrate this. There are two species of blue-throat, *Cyanecula suecica* and *C. leucocyana*: the former with its red-breast patch is abundant in Sweden in summer, but is never found in Germany, except most accidentally, as the other is the common form of Central Europe. Yet both are abundant in Egypt and Syria, where they winter, and I have on several occasions obtained both species out of the same flock. Hence we infer that the Swedish bird makes its journey from its winter quarters with scarcely a halt, while the other proceeds leisurely to its nearer summer quarters. On the other hand, I have more than once seen myriads of swallows, martins, sand-martins, and, later in the season, swifts, passing up the Jordan Valley and along the Bukoa of Central Syria, at so slight an elevation that I was able to distinguish at once that the flight consisted of swallows or house-martins. This was in perfectly calm clear weather. One stream of swallows, certainly not less than a quarter of a mile wide, occupied more than half an hour in passing over one spot, and flights of house-martins, and then of sand-martins, the next day, were scarcely less numerous. These flights must have been straight up from the Red Sea, and may have been the general assembly of all those which had wintered in East Africa. I cannot think that these flights were more than 1000 feet high. On the other hand, when standing on the highest peak in the Island of Palma, 6500 feet, with a dense mass of clouds beneath us, leaving nothing of land or sea visible, save the distant Peak of Tenerife, 13,000 feet, I have watched a flock of Cornish choughs soaring above us, till at length they were absolutely undistinguishable by us except with field-glasses.

As to the speed with which the migration flights are accomplished, they require much further observation. Herr Gäcke maintains that godwits and plovers can fly at the rate of 240 miles an hour (!), and the late Dr. Jerdon stated that the spine-tailed swift (*Acanthya caudatus*), roosting in Ceylon, would reach the Himalayas (1200 miles) before sunset. Certainly in their ordinary flight the swift is the only bird I have ever noticed to outstrip an express train on the Great Northern Railway.

Observation has shown us that, while there is a regular and uniform migration in the case of some species, yet that, beyond these, there comes a partial migration of some species, immigrants and emigrants simultaneously, and this, besides the familiar vertical emigration from higher to lower altitudes and *vice versa*, as in the familiar instance of the lapwing and golden plover. There is still much scope for the field naturalist in observation of these partial migrations. There are also species in which some individuals migrate and some are sedentary, e.g. in the few primeval forests which still remain in the Canary Islands, and which are enshrouded in almost perpetual mist, the wood-

cock is sedentary, and not uncommon. I have often put up the bird and seen the eggs; but in winter the number is vastly increased, and the visitors are easily to be distinguished from the residents by their lighter colour and larger size. The resident never leaves the cover of the dense forest, where the growth of ferns and shrubs is perpetual, and fosters a moist, rich, semi-peaty soil, in which the woodcock finds abundant food all the year, and has thus lost its migratory instincts.

But why do birds migrate? Observation has brought to light many facts which seem to increase the difficulties of a satisfactory answer to the question. The autumnal retreat from the breeding quarters might be explained by a want of sufficient sustenance as winter approaches in the higher latitudes, but this will not account for the return migration in spring, since there is no perceptible diminution of supplies in the winter quarters. A friend of mine, who was for some time stationed at an infirmary at Kikombo, on the high plateau south-east of Victoria Nyanza Lake, almost under the equator, where there is no variation in the seasons, wrote to me that from November to March the country swarmed with swallows and martins, which seemed to the casual observer to consist almost wholly of our three species, though occasionally a few birds of different type might be noticed in the larger flocks. Towards the end of March, without any observable change in climatic or atmospheric conditions, nine-tenths of the birds suddenly disappeared, and only a sprinkling remained. These, which had previously been lost amid the myriad of winter visitors, seemed to consist of four species, of which I received specimens of two, *Hirundo puella* and *H. senegalensis*. One, described as white underneath, is probably *H. athiopica*; and the fourth, very small, and quite black, must be a *Psittodrope*. All these remained through spring and summer. The northward movement of all the others must be through some impulse not yet ascertained. In many other instances observation has shown that the impulse of movement is not dependent on the weather at the moment. This is especially the case with sea birds. Prof. Newton observes that they can be trusted as the almanack itself. Foul weather or fair, heat or cold, the puffins, *Fratercula arctica*, repair to some of their stations punctually on a given day, as if their movements were regulated by clockwork. In like manner, whether the summer be cold or hot, the swifts leave their summer home in England about the first week in August, only occasional stragglers ever being seen after that date. So in three different years I noticed the appearance of the common swift (*Cypselus apus*) in myriads on one day in the first week in April. In the case of almost all the land birds, it has been ascertained by repeated observations that the male birds arrive some days before the hens. I do not think it is proved that they start earlier; but, being generally stronger than the females, it is very natural that they should outstrip their weaker mates. I think, too, that there is evidence that those species which have the most extended southerly range have also the most extended northerly range. The same may hold good of individuals of the same species, and may be accounted for, or account for, the fact that, e.g., the individuals of the wheatear or of the willow wren which penetrate furthest north have longer and stronger wings than those individuals which terminate their journey in more southern latitudes. The length of wing of two specimens of *Saxicola ananthe* in my collection from Greenland and Labrador exceeds by 6 inch the length of British and Syrian specimens, and the next longest, exceeding them by 5 inch, is from the Gambia. So the sedentary *Phylloscopus trochilus* of the Canaries has a perceptibly shorter wing than European specimens.

To say that migration is performed by instinct is no explanation of the marvellous faculty, it is an evasion of the difficulty. Prof. Möbius holds that birds crossing the ocean may be guided by observing the rolling of the waves, but this will not hold good in the varying storms of the Atlantic, still less in the vast stretch of stormy and landless ocean crossed by the bronze cuckoo (*Chrysococcyx lucidus*) in its passage from New Guinea to New Zealand. Prof. Palmén ascribes the due performance of the flight to experience, but this is not confirmed by field observers. He assumes that the flights are led by the oldest and strongest, but observation by Herr Gäcke has shown that among migrants, as the young and old journey apart and by different routes, the former can have had no experience. All ornithologists are aware that the parent cuckoos leave this country long before their young ones are hatched by their foster-parents. The sense of sight cannot guide birds which travel by night, or span oceans or continents in a single flight. In noticing all the phenomena of

migration, there yet remains a vast untitled region for the field naturalist.

What Prof. Newton terms the sense of direction, unconsciously exercised, is the nearest approach yet made to a solution of the problem. He remarks how vastly the sense of direction varies in human beings, contrasting its absence in the dwellers in towns compared with the power of the shepherd and the countryman, and, infinitely more, with the power of the savage or the Arab. He adduces the experience of Middendorff among the Samojeds, who know how to reach their goal by the shortest way through places wholly strange to them. He had known it among dogs and horses (as we may constantly perceive), but was surprised to find the same incomprehensible animal faculty unweakened among uncivilised men. Nor could the Samojeds understand his enquiry how they did it? They disarmed him by the question, How now does the arctic fox find its way aright on the Tundra, and never go astray? and Middendorff adds: "I was thrown back on the unconscious performance of an inherited animal faculty"; and so are we!

There is one more kind of migration, on which we know nothing, and where the field naturalist has still abundant scope for the exercise of observation. I mean what is called exceptional migration, not the mere wanderings of waifs and strays, nor yet the uncertain travels of some species, as the crossbill in search of food, but the colonising parties of many gregarious species, which generally, so far as we know in our own hemisphere, travel from east to west, or from south-east to north-west. Such are the waxwing (*Ampelis garrula*), the pastor stalling (*Pastor roseus*) and Pallas's sandgrouse, after intervals sometimes of many years, or sometimes for two or three years in succession. The waxwing will overspread Western Europe in winter for a short time. It appears to be equally inconstant in its choice of summer quarters, as was shown by J. Wolley in Lapland. The rose pastor regularly winters in India, but never remains to breed. For this purpose the whole race seems to collect and travel north-west, but rarely, or after intervals of many years, returns to the same quarters. Verona, Broussa, Smyrna, Odessa, the Dobrujscha have all during the last half-century been visited for one summer by tens of thousands, who are attracted by the visitations of locusts, on which they feed, rear their young, and go. These irruptions, however, cannot be classed under the laws of ordinary migration. Not less inexplicable are such migrations as those of the African darter, which, though never yet observed to the north of the African lakes, contrives to pass, every spring, unobserved to the lake of Antioch in North Syria, where I found a large colony rearing their young, which, so soon as their progeny was able to fly, disappeared to the south-east as suddenly as they had arrived.

There is one possible explanation of the sense of direction unconsciously exercised, which I submit as a working hypothesis. We are all aware of the instinct, strong both in mammals and birds without exception, which attracts them to the place of their nativity. When the increasing cold of the northern regions, in which they all had their origin, drove the mammals southward, they could not retrace their steps, because the increasing polar sea, as the arctic continent sank, barred their way. The birds reluctantly left their homes as winter came on, and followed the supply of food. But as the season in their new residence became hotter in summer, they instinctively returned to their birthplaces, and there reared their young, retiring with them when the recurring winter impelled them to seek a warmer climate. Those species which, unfitted for a greater amount of heat by their more protracted sojourn in the northern regions, persisted in revisiting their ancestral homes, or getting as near to them as they could, retained a capacity for enjoying a temperate climate, which, very gradually, was lost by the species which settled down more permanently in their new quarters, and thus a law of migration became established on the one side, and sedentary habits on the other.

If there be one question on which the field naturalist may contribute, as lion's provider to the philosopher, more than another, it is on the now much disputed topic of "mimicry," whether protective or aggressive. As Mr. Beddard has remarked on this subject, "The field of hypothesis has no limits, and what we need is more study"—and, may we not add, more accurate observation of facts. The theory of protective mimicry was first propounded by Mr. H. W. Bates, from his observations on the Amazon. He found that the group of butterflies, *Heliconiidae*, conspicuously banded with yellow and black, were provided with certain glands

which secrete a nauseating fluid, supposed to render them unpalatable to birds. In the sand districts he found also similarly coloured butterflies, belonging to the family *Pieridae*, which so closely resembled the others in shape and markings as to be easily mistaken for them, but which, unprovided with such secreting glands, were unprotected from the attacks of birds. The resemblance, he thought, was brought about by natural selection for the protection of the edible butterflies, through the birds mistaking them for the inedible kind. Other cases of mimicry among a great variety of insects have since been pointed out, and the theory of protective mimicry has gained many adherents. Among birds, many instances have been adduced. Mr. Wallace has described the extraordinary similarity between birds of very different families, *Oriolus bournensis* and *Philemon moluccensis*, both peculiar to the island of Bouru. Mr. H. O. Forbes has discovered a similar brown oriole, *Oriolus decipiens*, as closely imitating the appearance of the *Philemon timorlaensis* of Timor-laut. A similar instance occurs in Ceram. But Mr. Wallace observes that, while usually the mimicking species is less numerous than the mimicked, the contrary appears to be the case in Bouru, and it is difficult to see what advantage has been gained by the mimicry. Now, all the species of *Philemon* are remarkably sombre-coloured birds, and the mimicry cannot be on their side. But there are other brown orioles, more closely resembling those named, in other Moluccan islands, and yet having no resemblance to the *Philemon* of the same island, as may be seen in the case of the *Oriolus phaeochromus* and *Philemon gilolensis* from Gilolo. Yet the oriole has adopted the same livery which elsewhere is a perfect mimicry. May it not therefore be that we have, in this group of brown orioles, the original type of the family undifferentiated? As they spread east and south we may trace the gradation, through the brown striation of the New Guinea bird, to the lighter, green-tinged form of the West Australian and the green plumage of the Southern Australian, while westward the brilliant yellows of the numerous Indian and African species were developed, and another group, preferring high elevations, passing through the mountain ranges of Java, Sumatra, and Borneo, intensified the aboriginal brown into black, and hence were evolved the deep reds of the various species which culminate in the crimson of Formosa, *Oriolus ardens*, and the still deeper crimson of *O. trailli* of the Himalayas.

It is possible that there may be similarity without mimicry, and, by the five laws of mimicry as laid down by Wallace, very many suggested cases must be eliminated. We all know that it is quite possible to find between species of very different genera extraordinary similarity which is not mimetic. Take, for instance, the remarkable identity of coloration in the case of some of the African species *Macronyx* and the American *Sturnella*, or, again, of some of the African *Campophaga* and the American *Agelaius*. The outward resemblance occurs in both cases in the red as well as in the yellow-coloured species of all four groups. But we find that the *Macronyx* of America and the *Campophaga* of Africa, in acquiring this coloration, have departed widely from the plain colour found in their immediate relatives. If we applied Mr. Scudder's theory on insects, we must imagine that the prototype form has become extinct, while the mimicker has established its position. This is an hypothesis which is easier to suggest than either to prove or to disprove. Similar cases may frequently be found in botany. The strawberry is not indigenous in Japan, but in the mountains there I found a potentilla in fruit which absolutely mimicked the Alpine strawberry in the minutest particulars, in its runners, its blossoms, and fruit; but the fruit was simply dry pith, supporting the seeds and retaining its colour without shrinking or falling from the stalk for weeks—a remarkable case, we cannot say of unconscious mimicry, but of unconscious resemblance. Mimicry in birds is comparatively rare, and still rarer in mammals, which is not surprising when we consider how small is the total number of the mammalia, and even of birds, compared with the countless species of invertebrates. Out of the vast assemblage of insects, with their varied colours and patterns, it would be strange if there were not many cases of accidental resemblance. A strict application of Wallace's five laws would, perhaps, if all the circumstances were known, eliminate many accepted instances.

As to cases of edible insects mimicking inedible, Mr. Poulton admits that even unpalatable animals have their special enemies, and that the enemies of palatable animals are not indefinitely numerous.

Mr. Beddard gives tables of the results obtained by Weismann, Poulton, and others, which show that it is impossible to lay down any definite law upon the subject, and that the likes and dislikes of insect-eating animals are purely relative.

One of the most interesting cases of mimicry is that of the *Volucella*, a genus of *Diptera*, whose larvæ live on the larvæ of *Hymenoptera*, and of which the perfect insect closely resembles some species of humble-bee. Though this fact is unquestioned, yet it has recently given rise to a controversy, which, so far as one who has no claim to be an entomologist can judge, proves that while there is much that can be explained by mimicry, there is, nevertheless, a danger of its advocates pressing it too far. *Volucella bombylans* occurs in two varieties, which prey upon the humble-bees, *Bombus muscorum* and *B. lapidarius*, which they respectively resemble. Mr. Bateson does not question the behaviour of the *Volucella*, but states that neither variety specially represents *B. muscorum*, and yet that they deposit their eggs more frequently in their nests than in the nests of other species which they resemble more closely. He also states that in a show case in the Royal College of Surgeons, to illustrate mining, two specimens of another species, *B. sylvarum*, were placed alongside of the *Volucella*, which they do resemble, but were labelled *B. muscorum*.

But Mr. Hart explains the parasitism in another way. He states that a test of *B. muscorum* is made on the surface, without much attempt at concealment, and that the bee is a peculiarly gentle species, with a very feeble sting; but that the species which the *Volucella* most resemble are irascible, and therefore more dangerous to intruders. If this be so, it is difficult to see why the *Volucella* should mimic the bee, which it does not affect, more closely than the one which is generally its victim. I do not presume to express any opinion further than this, that the instances I have cited show that there is much reason for further careful observation by the field naturalist, and much yet to be discovered by the physiologist and the chemist, as to the composition and nature of animal pigments.

I had proposed to occupy a considerable portion of my address with a statement of the present position of the controversy on heredity, by far the most difficult and important of all those subjects which at present attract the attention of the biologist; but an attack of illness has compelled me to abandon my purpose. Not that I proposed to venture to express any opinions of my own, for, with such protagonists in the field as Weismann, Wallace, Romanes, and Poulton on the one side, and Herbert Spencer and Hartog on the other, "*Non nostrum inter vos tantas componere lites*."

So far as I can understand Weismann's theory, he assumes the separation of germ cells and somatic cells, and that each germ cell contains in its nucleus a number of "ids," each "id" representing the personality of an ancestral member of the species, or of an antecedent species. "The first multicellular organism was probably a cluster of similar cells, but these units soon lost their original homogeneity. As the result of mere relative position, some of the cells were especially fitted to provide for the nutrition of the colony, while others undertook the work of reproduction." The latter, or germ-plasm, he assumes to possess an unlimited power of continuance, and that life is endowed with a fixed duration, not because it is contrary to its nature to be unlimited, but because the unlimited existence of individuals would be a luxury without any corresponding advantage.

Herbert Spencer remarks upon this: "The changes of every aggregate, no matter of what kind, inevitably end in a state of equilibrium. Suns and planets die, as well as organisms." But has the theory been proved, either by the histologist, the microscopist, or the chemist? Spencer presses the point that the immortality of the protozoa has not been proved. And, after all, when Weismann makes the continuity of the germ plasm the foundation of a theory of heredity, he is building upon a pure hypothesis.

From the continuity of the germ-plasm, and its relative segregation from the body at large, save with respect to nutrition, he deduces, *a priori*, the impossibility of characters acquired by the body being transmitted through the germ-plasm to the offspring. From this he implies that where we find no intelligible mechanism to convey an imprint from the body to the germ, there no imprint can be conveyed. Romanes has brought forward many instances which seem to contradict this theory, and Herbert Spencer remarks that "a recognised principle of reasoning—the law of parsimony—forbids the

assumption of more causes than are needful for the explanation of phenomena. We have evident causes which arrest the cell multiplication, therefore it is illegitimate to ascribe this arrest to some property inherent in the cells."

With regard to the reduction or disappearance of an organ, he states "that when natural selection, either direct or reversed, is set aside, why the mere cessation of selection should cause decrease of an organ, irrespective of the direct effects of disease, I am unable to see. Beyond the production of changes in the size of parts, by the selection of fortuitously arising variation, I can see but one other cause for the production of them—the competition among the parts for nutriment. . . . The active parts are well supplied, while the inactive parts are ill supplied and dwindle, as does the arm of the Hindu fakir. This competition is the cause of economy of growth—this is the cause of decrease from disease."

I may illustrate Mr. Herbert Spencer's remarks by the familiar instance of the pinions of the Kakapo (*Stringops*)—still remaining, but powerless for flight.

As for acquired habits, such as the modification of bird architecture by the same species under changed circumstances, how they can be better accounted for than by hereditary transmitted instinct, I do not see. I mean such cases as the ground-nesting *Didunculus* in Samoa having saved itself from extinction since the introduction of cats, by roosting and nesting in trees; or the extraordinary acquired habit of the black-cap in the Canaries, observed by Dr. Lowe, of piercing the calyx of *Hibiscus rosasinensis*—an introduced plant—to attract insects, for which he quietly sits waiting. So the lying low of a covey of partridges under an artificial kite would seem to be a transmitted instinct from a far-off ancestry not yet lost; for many generations of partridges, I fear, must have passed since the last kite hovered over the forefathers of an English partridge, save in very few parts of the island.

I cannot conclude without recalling that the past year has witnessed the severance of the last link with the pre-Darwinian naturalists in the death of Sir Richard Owen. Though never himself a field-worker, or the discoverer of a single animal living or extinct, his career extends over the whole history of palæontology. I say palæontology, for he was not a geologist in the sense of studying the order, succession, area, structure, and disturbance of strata. But he accumulated facts on the fossil remains that came to his hands, till he won the fame of being the greatest comparative anatomist of the age. To him we owe the building up of the skeletons of the giant *Dinornithide* and many other of the perished forms of the gigantic sloths, armadillos, and mastodons of South America, Australia, and Europe. He was himself a colossal worker, and he never worked for popularity. He had lived and worked too long before the Victorian age to accept readily the doctrines which have revolutionised that science, though none has had a larger share in accumulating the facts, the combination of which of necessity produced that transformation. But, though he clung fondly to his old idea of the archetype, no man did more than Owen to explode the rival theories of both Wernerians and Huttonians, till the controversies of Plutonians and Neptunians came to us from the far past with as little to move our interest as the blue and green controversies of Constantinople.

Nor can we forget that it is to Sir Richard's indomitable perseverance that we owe the magnificent palace which contains the national collections in Cromwell Road. For many years he fought the battle almost alone. His demand for a building of two stories, covering five acres, was denounced as audacious. The scheme was pronounced foolish, crazy, and extravagant; but, after twenty years' struggle, he was victorious, and in 1872 the Act was passed which gave not five, but more than seven acres for the purpose. Owen retired from his direction in 1883, having achieved the crowning victory of his life. Looking back in his old age on the scientific achievements of the past, he fully recognised the prospects of still further advances, and observed, "The known is very small compared with the knowable, and we may trust in the Author of all truth, who, I think, will not let that truth remain for ever hidden."

I have endeavoured to show that there is still room for all workers, that the naturalist has his place, though the morphologist and the physiologist have rightly come into far greater prominence, and we need not yet abandon the field-glass and the lens for the microscope and the scalpel. The studies of the laboratory still leave room for the observations of the field. The investigation of muscles, the analysis of brain tissue, the

research into the chemical properties of pigment, have not rendered worthless the study and observation of life and habits. As you cannot diagnose the Red Indian and the Anglo-Saxon by a comparison of their respective skeletons or researches into their muscular structure, but require to know the habits, the language, the modes of thought of each; so the mammal, the bird, and even the invertebrate, has his character, his voice, his impulses, aye, I will add, his ideas, to be taken into account in order to discriminate him. There is something beyond matter in life, even in its lowest forms. I may quote on this the caution uttered by a predecessor of mine in this chair (Prof. Milnes Marshall): "One thing above all is apparent, that embryologists must not work single-handed; must not be satisfied with an acquaintance, however exact, with animals from the side of development only; for embryos have this in common with maps, that too close and too exclusive a study of them is apt to disturb a man's reasoning power."

The ancient Greek philosopher gives us a threefold division of the intellectual faculties—*φρόνις*, *ἐπιστήμη*, *σύνεσις*—and I think we may apply it to the subdivision of labour in natural science: *φρόνις*, ἡ τὰ καθ' ἑκάστη γνῶσις, is the power that divides, discerns, distinguishes—i.e. the naturalist; *σύνεσις*, the operation of the closest zoologist, who investigates and experiments; and *ἐπιστήμη*, the faculty of the philosopher, who draws his conclusions from facts and observations.

The older naturalists lost much from lack of the records of previous observations; their difficulties were not ours, but they went to nature for their teachings rather than to books. Now we find it hard to avoid being smothered with the literature on the subject, and being choked with the dust of libraries. The danger against which Prof. Marshall warns the embryologist is not confined to him alone; the observer of facts is equally exposed to it, and he must beware of the danger, else he may become a mere materialist. The poetic, the imaginative, the emotional, the spiritual, all go to make up the man; and if one of these is missing, he is incomplete.

I cannot but feel that the danger of this concentration upon one side only of nature is painfully illustrated in the life of our great master, Darwin. In his early days he was a lover of literature, he delighted in Shakespeare and other poets; but after years of scientific activity and interest, he found on taking them up again that he had not only grown indifferent to them, but that they were even distasteful to him. He had suffered a sort of atrophy on that side of his nature, as the disused pinions of the Kakapo have become powerless—the spiritual, the imaginative, the emotional, we may call it.

The case of Darwin illustrates a law—a principle we may call it—namely, that the spiritual faculty lives or dies by exercise or the want of it even as does the bodily. Yet the atrophy was unconscious. Far was it from Darwin to ignore or depreciate studies not his own. He has shown us this when he prefixed to the title-page of his great work the following extract from Lord Chancellor Bacon: "To conclude, therefore, let no man, out of a weak conceit of sobriety, or an ill-applied moderation, think or maintain that a man can search too far, or be too well studied in the book of God's word, or in the book of God's works, divinity or philosophy, but rather let men endeavour an endless progress or proficience in both." In true harmony this with the spirit of the father of natural history, concluding with the words, "O Lord, how manifold are Thy works, in wisdom hast Thou made them all, the earth is full of Thy riches."

SECTION G.

MECHANICAL SCIENCE.

OPENING ADDRESS BY JEREMIAH HEAD, M.Inst.C.E.,
PAST PRES. INST. MECH. E., F.C.S., PRESIDENT OF THE
SECTION.

THIS Section of the British Association for the Advancement of Science was founded with the object of making more widely known, and more generally appreciated, all well-ascertained facts and well-established principles having special reference to mechanical science.

As President of the Section for the year, it becomes my duty to inaugurate the proceedings by addressing you upon some portion of the scientific domain to which I have referred, and in which your presence here indicates that you are all more or less interested.

Mechanical Science.

The founders of the British Association no doubt regarded the field of operations which they awarded to Section G as a not less purely scientific one than those which they allotted to the other Sections. And indeed, mechanical science studied, say, by Watt was as free from suspicion of commercial bias as chemical science studied, say, by Faraday.

But whatever may have been the original idea, the practice of the Section has recently been to expend most of its available time in the consideration of more or less beneficial applications of mechanical science, rather than of the first principles thereof. Our Section has become more and more one of applied rather than of pure science. None of the other Sections is free from this fault, if fault it be (which I do not contend or admit), but Section G seems to me to be beyond all question, and beyond all others, the Section of applied science.

The charter of the Institution of Civil Engineers commences by reciting that the object of that society is "the general advancement of mechanical science, and more particularly for promoting the acquisition of that species of knowledge which constitutes the profession of a civil engineer, being the art of directing the great sources of power in nature for the use and convenience of man."

It seems that in 1828, when the Institution was incorporated, the term "mechanical science" had a wider meaning than it is now usually understood to have. For, according to the charter, the art of directing the great sources of power in nature is only a particular species of knowledge which "mechanical science" includes.

In 1836, or eight years later, the founders of our Section adopted the term without again defining it. Probably they accepted the careful definition of the Great George Street Institution. Time has shown the wisdom of that decision. For we civil engineers and other frequenters of Section G in active practice need far more knowledge than mechanical science can teach us in the ordinary or narrow sense of the term. Our art in its multifarious branches requires, if success is to be attained, the acquisition and application of almost all the other sciences which belong to the fields of research relegated to the other Sections. For how could the gigantic engineering structures of modern times be designed without recourse to mathematics, or steam and other motors without a knowledge of physics, or modern metallurgical operations be conducted without chemistry, or mining without geology, or communications by rail, ship, and wire be established and carried on with all parts of the world without attention to geography, or extensive manufacturing enterprises be developed if the laws of economics were neglected?

As to biological studies, they seem at first sight to have but little to do with mechanical science. It might even be thought that the civil engineer could afford altogether to neglect this part of the work of the Association. But I trust I shall be able to show you before I finish that any such view is absolutely untenable.

Mechanisms in Nature.

Indeed, I hope, in the course of this address, to satisfy you that mechanical science is largely indebted to mechanisms as they exist in nature, if not for its origin, at all events for much of its progress hitherto, and that nature must still be our guide.

Mechanical science has been built up entirely upon observation and experiment, and the natural laws which have been induced therefrom by man. The lower animals in their wild condition work with tools or appliances external to their bodies to but a very slight extent, and man in a primitive or savage state does the same. But many, if not most, animals can be taught to use mechanisms if carefully trained from infancy. Thus, the well-known donkey at Carisbrooke Castle draws water from a deep well by a treadmill arrangement just as well as a man could do it. He watches the rope on the barrel till the full pail rises above the parapet of the well, then slacks back a little to allow it to be rested thereon, and only then leaves the drum and retreats to his stable. But, according to his attendant, four years were needed for his education, and unless it had been commenced early it would have been useless.

I have seen a canary gradually lift from a little well, situated a foot below its perch, a thimble full of water by pulling up with its beak, bit by bit, a little chain attached to it, and securing each length lifted with its foot till it could take another pull. When the thimble reached its perch level the bird took a drink,

and then let it fall back into the well. Numerous other examples will doubtless occur to you.

But though animals can be taught to make use of mechanical appliances provided for them—a fact which shows the existence in their brains of a faculty corresponding in kind, if not in degree, to the mechanical faculty in man—they rarely, on their own initiative, make use of anything external to their bodies as tools; and still more rarely, if ever, do they make, alter, or adapt such mechanical aids. Mr. C. Wood, of Middlesbrough, informs me that certain crows which frequent oyster beds on the coast of India, wait until the receding tide uncovers the oysters, which still remain open for a time. A crow will then put a pebble inside one, and, having thus gagged it and secured its own safety, will proceed to pick it out and eat it at leisure. A monkey will crack a nut between two stones, and will hurl missiles at his enemies. But in some countries he is systematically entrapped by tying to a tree a hollow gourd containing rice, and having a hole large enough for his hand, but too small for his clenched fist, to pass through. He climbs the tree and grasps the rice, and remains there till taken, being too greedy, and not having sufficient sense, to let go the rice and withdraw his hand.

This is on a par with the snuff-taking imbecile, described by Hugh Miller ("My Schools and Schoolmasters"), whom the boys used to tease by giving him a little snuff at the bottom of a deep tin box. The imbecile would try to get at it for hours without the idea ever occurring to him that he might achieve his object by turning the box upside down.

All animals are, however, in their bodily frames, and in the intricate processes and functions which go on continuously therein, mechanisms of so elaborate a kind that we can only look and wonder and strive to imitate them a little here and there. The mechanism of their own bodily frames is that with which the lower animals have to be content, and whilst they are in the prime of life and health, and in their natural environment, it is generally sufficient for all their purposes. Man has a still more perfect, or rather a still more versatile bodily mechanism, and one which in a limited environment would be equally sufficient for his needs. But he has also an enterprising and powerful mind which impels him to strive after and enables him to enjoy fields of conquest unknown to, and uncared for by, the relatively brainless lower animals.

Urged on by these superior mental powers, man must soon have perceived that by the use of instruments he could more quickly and easily gain his ends, and he would not be long in discovering that certain other animals, such as the ox and the horse, were teachable and his willing slaves, provided only he fed and trained them, and treated them kindly.

First, in common with other animals, he would find out that stones and sticks were of some use as weapons and tools; then he would go further and utilise skins and thongs for clothing and harness; and by selecting and modifying his stones and sticks he would form them into rough implements, which would enable him to cut down trees and to make rude huts and boats. Animals caught and domesticated would first be taught to haul light logs along the ground, then to move heavier ones on rollers; and later, in order to avoid the necessity for continual replacement on the rollers, the wheel and axle would be gradually developed.

The mechanical nomenclature of all languages is largely derived from the bodies of men and other animals. From this it is clear that animals have always been recognised as mechanisms, or as closely related thereto. The names borrowed from them generally indicate a resemblance in form rather than in function, though not invariably so.

Thus in our own language we have the "head" of a ship, a river, a lake, a jetty, a bolt, a nail, a screw, a rivet, a flight of stairs, and a column of water; the brow of an incline; the crown of an arch; the toe of a pier; the foot of a wall; the forefoot, heel, ribs, waist, knees, skin, nose, and dead eyes of a ship; also turtle backs and whale backs; the jaws of a vice; the claws of a clutch; the teeth of wheels; necks, shoulders, eyes, nozzles, legs, ears, mouths, lips, cheeks, elbows, feathers, tongues, throats, and arms; caps, bonnets, collars, sleeves, saddles, gussets, paddles, fins, wings, horns, crabs, donkeys, monkeys, and dogs; flywheels, running nooses, crane necks, grasshopper engines, &c.

Not only has our mechanical nomenclature been largely taken from animals, but many of our principal mechanical devices have pre-existed in them. Thus, examples of levers of all

three orders are to be found in the bodies of animals. The human foot contains instances of the first and second, and the forearm of the third order of lever. The patella, or knee-cap, is practically a part of a pulley. There are several hinges and some ball-and-socket joints, with perfect lubricating arrangements. Lungs are bellows, and the vocal organs comprise every requisite of a perfect musical instrument. The heart is a combination of four force-pumps acting harmoniously together. The wrist, ankle, and spinal vertebrae form universal joints. The eyes may be regarded as double lens cameras, with power to adjust focal length, and able, by their stereoscopic action, to gauge size, solidity, and distance. The nerves form a complete telegraph system with separate up and down lines and a central exchange. The circulation of the blood is a double-line system of canals, in which the canal liquid and canal boats move together, making the complete circuit twice a minute, distributing supplies to wherever required, and taking up return loads wherever ready without stopping. It is also a heat distributing apparatus, carrying heat from wherever it is generated or in excess to wherever it is deficient, and establishing a general average, just as engineers endeavour, but with less success, to do in houses and public buildings. The respiratory system may be looked upon as that whereby the internal ventilation of the bodily structure is maintained. For by it oxygen is separated from the air and imparted to the blood for conveyance and use where needed, whilst at the same time the products of combustion are extracted therefrom and discharged into the atmosphere.

Mastication, which is the first process in the alimentary system, is, or rather should be, a perfect system of cutting up and grinding, and to assist and save animal, and especially human, mastication is the chief aim and object of all the gigantic milling establishments of modern times. The later alimentary processes are rather chemical than mechanical, but still the successive muscular contractions, whereby the contents of the canal are forced through their intricate course, are distinctly mechanical, and may have suggested the action of various mechanisms which are used in the arts to operate on plastic materials, and cause them to flow into new forms and directions.

The superiority of man to the lower animals can only have become conspicuous and decided when he began to use his inventive faculties and to fashion weapons and implements of a more efficient kind than the sticks and stones which they also occasionally use.

But human races and individuals were never equally endowed by nature. Some individuals would have greater inventive powers than others, and these and their posterity would gradually become dominant races. Large masses of mankind are still more or less in the position of primeval man, which, if we accept the conclusions of Darwin, Lubbock, and other modern men of science, we must regard as one of barbarism. For they are still without tools, appliances, and clothes, except of the most elementary kinds, and mechanical science might almost be non-existent, so far as they are concerned.¹

It would obviously be impossible for me to treat of or call attention even to an infinitesimal extent to the results of mechanical science which surround us now so profusely, and which make our life so different from that of primeval man; and, even if it were possible, it would be quite unnecessary. We have all grown up in a mechanical age. We are so familiarised with artificial aids that we have come to regard them as part of our natural environment, and their occasional absence impresses us far more than their habitual presence.

I propose, with your leave, to proceed to the consideration of how far man is, in his natural condition, and has become by aid of mechanical science, able to compete successfully with other and specially endowed animals, each in its own sphere of action.

Bodily Powers of Man and other Animals.

The bodily frame of man is adapted for life and movement only on or near to the surface of the earth. Without mechanical aids he can walk for several hours, at a speed which is ordinarily from 3 to 4 miles per hour. Under exceptional circumstances he has accomplished over 8 miles ("Whitaker's Almanack," 1893, p. 395) in one hour, and an average of

¹ Mr. H. L. Lapage, M.Inst.C.E., who has just returned from Western Australia, states that he found the natives of both sexes and all ages absolutely nude.

2½ miles per hour for 141 hours.¹ In running he has covered about 11½ miles in an hour. In water he has proved himself capable of swimming 100 yards at the rate of 3 miles per hour, and 22 miles at rather over 1 mile per hour. He can easily climb the most rugged mountain path and descend the same. He can swim up a bare pole or a rope, and when of suitable physique and trained from infancy can perform those wonderful feats of strength and agility which we are accustomed to expect from acrobats. He has shown himself able to jump as high as 6 feet 2½ inches from the ground, and over a horizontal distance of 23 feet 3 inches, and has thrown a cricket-ball as far as 382½ feet before it struck the ground. (*Chambers' Encyclopedia*, "Athletic Sports.")

The attitude and action of a man in throwing a stone or a cricket-ball, where he exerts a considerable force at several feet from the ground, to which the reaction has to be transmitted and to which he is in no way fastened, are unequalled in any artificial machine. The similar but contrary action of pulling a rope horizontally, as in "tug of war" competitions, is equally remarkable.

So also the power of the living human mechanism to withstand widely diverse and excessive strains is altogether unapproachable in artificial constructions. Thus, although fitted for an external atmospheric pressure of about 15 lbs. to the square inch, he has been enabled, as exemplified by Messrs. Glaisher and Coxwell in 1862, to ascend to a height of seven miles, and breathe air at a pressure of only 3½ lbs. per square inch, and still live. And, on the other hand, divers have been down into water 80 feet deep, entailing an extra pressure of about 36 lbs. per square inch, and have returned safely. One has even been to a depth of 150 feet, but the resulting pressure of 67 lbs. per square inch cost him his life. (*Pall Mall Gazette*, July 5, 1893, p. 8.)

Recent fasting performances (if the published records are to be trusted) are not less remarkable when we are comparing the human body as a piece of mechanism with those of artificial construction. For what artificial motor could continue its functions forty days and nights without fuel, or, if the material of which it was constructed were gradually consumed to maintain the flow of energy, could afterwards build itself up again to its original substance?

These and other performances are, when considered individually and separately, often largely exceeded by other animals specially adapted to their own limited spheres of activity. The marvel is not, therefore, that the human bodily mechanism is capable of any one kind of action, but that, in its various developments, it can do all or any of them, and also carry a mind endowed with far wider powers than any other animal.

Animals other than man are also adapted for life and movement on or about the surface of the earth. This includes a certain distance below the ground, as in the case of earthworms; under the water, as in the case of fish; on the water, as in the case of swimming birds; and in the air, as with flying birds.

As far as I know, no animal burrows downwards into the earth to a greater depth than 8 feet ("Vegetable Mould and Earthworms," by Charles Darwin, p. 111), and then only in dry ground. Man is naturally very ill-adapted for boring into the earth as the earthworm does. Indeed, without mechanical aids he would be helpless in excavating or in dealing with the accumulations of water which are commonly met with underground. But by aid of the steam engine for pumping, for air-compressing, ventilating, hauling, rock-boring, electric lighting, and so forth, and by the utilisation of explosives, he has obtained a complete mastery over the crust of the earth and its mineral contents, down to the depth where, owing to the increase of temperature, the conditions of existence become difficult to maintain.

I have said that on land, man, unaided by mechanism, has been able to cover about 11½ miles in one hour. Two miles he has been able to run at the rate of nearly 13 miles per hour, and 100 yards at the rate of over 20 miles per hour. (*Chambers' Encyclopedia*, "Athletic Sports.") But the horse, though he cannot walk faster than man, nor exceed him in jumping heights or distances, can certainly beat him altogether when galloping or trotting. A mile has been galloped in 103 seconds, equal to 35 miles per hour; and has been trotted in 124 seconds, equal to 29 miles per hour. (*Chambers' Encyclopedia*, "Horse.")

There are many other animals, such as ostriches, greyhounds, antelopes, and wolves, which run at great speeds, but reliable

records are difficult to obtain, and are scarcely necessary for our present purpose.

Mechanical Aid without Extraneous Motive-power.

Let us now consider how man's position as a competitor with other animals in speed is affected by his use of mechanical aids, but without any extraneous motive-power.

Locomotion on Land.—Where there is a stretch of good ice, and he is able to bind skates on his feet, he can thereby largely augment his running speed. This was exemplified by the winner of the match for amateurs at Haarlem last winter, who accomplished the distance of 3¼ miles at the rate of about 21 miles per hour.

But the most wonderful increase to the locomotive power of man on land is obtained by the use of the modern cycle. Cycling is easily performed only where roads, wind, and weather are favourable. But similar conditions must also be present to secure the best speed of horses, with which we have been making comparison. One mile has been cycled at the rate of 27¼ miles per hour ("Whitaker's Almanack," 1893), 50 at 20, 100 at 16½ (*Chambers' Encyclopedia*, "Cycling"), 388 at 12½ (*Times*, September 26 to October 7, 1892), and 900 at 12¼ ("Whitaker's Almanack," 1893), miles per hour.

The recent race between German and Austrian cavalry officers on the high road between Vienna and Berlin has afforded an excellent opportunity to judge of the speed and endurance of horses as compared with men over long distances. Count Starhemberg, the winner, performed the distance, about 388 miles, in 71¾ hours, equal to 5¼ miles per hour. He rested only one hour in twelve. His horse, though successful, has since died. (Vienna Berlin Race, June, 1893.)

Lawrence Fletcher cycled, also along the high roads, from Land's End to John o' Groat's house, 900 miles, in 72¼ hours, equal to 12¼ miles per hour, or more than double the distance that the Count rode, and at above double the speed. To the best of my knowledge he still lives, and is no worse for his effort. The horse in this case would have to carry extra weight equal to one-sixth of his own, and the cyclist equal to a quarter of his own. But the horse carried himself and his rider on his own legs, while the cyclist made his machine bear the weight of itself and rider. Herein was probably the secret of his easy victory.

With the very remarkable exception of long-distance cycling, which is of limited application, man, relying on his own bodily strength, cannot successfully compete with other animals which, like the horse, are specially fitted for rapid land locomotion. His only alternatives are either to utilise the horse and ride or drive him, and so get the benefit of his superior strength and speed, or to use his own inventive faculty and construct appliances altogether apart from animal mechanisms. In either case he virtually gives up the contest as a self-moving animal, and to a great extent abandons himself to be carried by others or by inanimate machinery.

Nearly seventy years ago mankind came to this conclusion, and the modern railway system is the result. The locomotive will go at least double the speed of the race-horse. It will carry not only itself, but three or four times its own weight in addition, and will go, not two or three, but 100 miles or more without stopping, if only the road ahead be clear. And the iron horse is fed and controlled without even so much exertion as that put forth by a man on a horse of flesh and bone.

Locomotion in Water.—Let us now consider the powers of man relatively to other animals in moving upon and through the great waters with which three-fourths of the earth's surface is covered. Here he is in competition with fishes, aquatic mammals, and swimming birds.

I have already stated that, unaided by mechanism, he has shown himself able to swim for short distances at the rate of three, and long distances (22 miles) at the rate of one mile per hour. He has also given instances of being able to remain under water for 4½ minutes. ("Whitaker's Almanack," 1893.)

Credible eye-witnesses inform me that porpoises easily overtake and keep pace with a steamer going 12½ knots, or, say, over 14 miles per hour, for an indefinite length of time. This is five and fifteen times the maximum swimming speed of a man for short and long distances respectively. No doubt the form and surface of a fish, whose main business is swimming, offer less resistance, and his muscular power is more concentrated and better applied towards propulsion in water than is the case with man, whose body is also adapted for so many other purposes.

¹ Recent pedestrian race from Berlin to Vienna.

I am further informed by Mr. Nelson, of Redcar, a naturalist who has made the experiment, that it is impossible for an ordinary sea-boat rowed by two men, and going at five miles per hour, to overtake the aquatic bird called the Great Northern Diver, when endeavouring to make his escape by alternately swimming on the surface and diving below. His speed is therefore nearly double the short and five times the long distance speed of unaided man in water. As regards remaining under water, fishes properly so-called have unlimited powers, and even aquatic mammals, such as whales, can remain under for 1½ hours.

Using only his own strength, but assisting himself with mechanical devices, man has been able to increase considerably his speed as a swimming animal. Mr. John McCall, of Walthamstow, informs me that in 1868 he constructed and repeatedly used an apparatus which acted like the tail of a fish. It consisted of a piece of whalebone, having a broad yet thin and elastic blade, tapering into a shank like the end of an oar. The blade was 15 inches wide and 4 feet long, including the shank. To the end of the latter a horizontal cross-bar 13 inches long was fitted, and leather pockets were provided at the ends for the feet. By swimming on his back and striking out alternately with his legs, he was able, with the assistance of this apparatus, to keep up with a sea boat pulled by two men at about 4 miles per hour.

By means of boats, which he propels by oars or sculls, and notwithstanding the increased weight, and therefore displacement, involved by them, man has been able to increase his speed on the surface of the water to a maximum of about 12 miles per hour for about 4 miles distance, under favourable circumstances. So, by supplementing his bodily powers by means of mechanical aids, such as the diving-bell and the diving-helmet, dress, and air-pump, or by the portable self-acting apparatus used with such good effect in the construction of the Severn tunnel, man has been able to approach very nearly to the natural diving powers of, at all events, aquatic mammals, except that he cannot move about in subaqueous regions with anything approaching their ease and celerity.

Invariably on water, as almost invariably on land, man is quite unable to compete in power of locomotion with other specially adapted animals, whether or not he avails himself of mechanical aids, so long as his own bodily strength is the only motive-power he employs. He has gradually come to recognise this fact, and to see that he must use this inventive faculties and find new and powerful motors external to himself if he would really claim to dominate the great waters of the earth.

The fastest mechanism of any size, animal or man-made, which, as far as I know, has ever cut its way through the waters for any considerable distance is the torpedo-boat, *Ariete*, made by Messrs. Thornycroft and Son, of London, in 1887. It has a displacement or total weight of about 110 tons, and machinery capable of exerting 1290 effective horse-power, or 11·7 horse-power per ton of weight or displacement; or, to put it in another form, an effective horse-power is by it obtained from a weight of 191 lbs., which includes vessel, machinery, fuel, stores, and attendants. The speed accomplished at the trials of this little craft, being the average of six one-mile tests, was 26·18 knots, or 30·16 miles per hour (*Engineering*, July 15, 1887). As might be expected, it resembles a fish, in that its interior is almost exclusively devoted to the machinery and accessories necessary for propulsion. During the trials the water, fuel, stores, and other ponderable substances carried amounted to 17·35 tons. Two similar boats were able to make the voyage to South America by themselves, though at much lower speed and replenishing their fuel on the way. No fish or swimming bird can match this performance. And inasmuch as 191 lbs. of dead weight produced 1 horse-power, as compared with from 150 to 250 lbs. in certain flying birds, it would seem that with suitable adaptations the *Ariete* might even have been made to navigate the air instead of the water.¹ But I will revert to this subject later on.

Where safety in any weather, and passenger and cargo carrying powers are aimed at, as well as, or prior to, the utmost attainable speed—and these must ever be the leading features of ocean-transit steamers if they are to attain commercial success—there I must refer you to those magnificent examples of naval

architecture which are more or less familiar to you all, and of which we, as a maritime nation, are so justly proud. If, for example, we turn our attention for a moment to the new Cunard liners, the *Campania* and *Lucania*, having each a weight or displacement of 18,000 tons and 24,000 effective horse-power, or 1·33 horse-power per ton of displacement, we shall find that, with the commercial advantages alluded to, they obtain a maximum speed of 22·5 knots, or about 26 miles per hour.

If, instead of 1·33 effective horse-power per ton of displacement, they were provided with eight times that amount, or 10·64 horse-power per ton, thereby sacrificing passenger and cargo accommodation and making them nearly as full of propelling machinery as the *Ariete* torpedo-boat, and if it were then found possible to apply this enormous power effectively, then there is every reason to believe they would accomplish for short distances double the speed, or, say, 45 knots, or about 52 statute miles per hour.

By inventing and utilising mechanical contrivances entirely independent of his own bodily strength, man can now pass over the surface of the waters at the rate of over 500 knots per day, and at the same time retain the comforts and conveniences of life as though he were on shore. He has in this way beaten the natural and specially fitted denizens of the deep in their own element, as regards speed and continuity of effort. But he is still behind them as to safety. We do not find that fishes or aquatic mammals often perish in numbers, as man does, by collisions in fogs, or by being cast on lee shores and rocks by stress of weather. Shall we ever arrive at the point of making ocean travelling absolutely safe? The Cunard Company is able to boast that from its commencement, fifty-three years ago, it has never lost a passenger's life or a letter, a statement which gives ground for hope that almost absolute safety is attainable. But, on the other hand, other owners of almost equal repute (not excluding the British Admiralty) are ever and anon losing magnificent vessels on rocks, in collisions, by fire, and even by stress of weather, in a way which makes us doubt whether it is possible for Britannia or any one else really to "rule the waves."

In one way the chances of serious disaster have been of late largely diminished, and here, again, Nature has been our teacher. The bodies of all animals except the very lowest are symmetrically formed on either side of a central longitudinal plane. Each important limb is in duplicate, and if one side is wounded the other can still act. We have at last found out the enormous advantage and increased safety of having the whole of our ship-propelling machinery in duplicate, and our ships made almost unsinkable by one longitudinal and numerous transverse bulkheads.

Locomotion in Air.—I now come to consider what is the position of man as regards locomotion in and through the great atmospheric envelope which surrounds the earth, in comparison with animals specially fitted by Nature for such work.

Nature seems never to bestow all her gifts on one individual or class of animals, and she never leaves any entirely destitute. For instance, the serpent, having no limbs whatever, would seem at first sight to be terribly handicapped; yet, in the language of the late Prof. Owen, "it can out-climb the monkey, out-swim the fish, out-leap the jerboa, and, suddenly loosing the close coils of its crouching spiral, it can spring in the air and seize the bird on the wing." (Pettigrew on "Animal Locomotion"). Here we have the spiral spring in nature before it was devised by man.

Flying animals seem to conform remarkably to this law. Thus we have birds like the penguin, which dive and swim but cannot fly; others, like the gannet, which dive, swim, fly, and walk; others, like the ostrich, which run, but can neither fly nor swim; and numberless kinds which can fly well, but have only slight pedestrian powers.

Man, unaided by mechanism, can, as we have seen, walk, run, swim, dive, and jump, and perform many remarkable feats; but for flying in the air he is absolutely unfitted. All his attempts (and there have been many) have up to the present been unsuccessful, whether or not he has availed himself of mechanical aids to his own bodily powers. It is said that a certain man fitted himself with apparatus in the time of James VI. of Scotland, and actually precipitated himself from the cliff below Stirling Castle, in sight of the king and his courtiers; but the apparatus collapsed, and he broke his leg, and that was the end of the experiment.

¹ M. Normand, of Havre, is building for the French Government two torpedo-boats, each having a displacement of 125 tons and 2717 effective horse-power, or 21·7 horse-power per ton of displacement. This is equivalent to 1 horse-power per 103 lbs., and is still within the limits of weight permissible for aerial flight. (See *Times*, June 19, 1893.)

But why should not man fly? It is not that he does not desire to do so. For every denizen of our precarious British climate, when he has noticed the ease with which swallows and other migratory birds fly off on the approach of winter, hundreds and even thousands of miles to the sunny south, must have wished he could do the same. One reason why we cannot fly, even with artificial aids, such as wings, is that, as in the case of the penguin or the ostrich, our bodily mechanism is specialised and our muscular power diffused in other directions, so that we could not actuate wings of sufficient area to carry us even if we had them.

M. de Lucy, a French naturalist, has shown that the wing-area of flying animals varies from about 49 square feet per lb. of weight in the gnat, and 5 square feet in the swallow, to half a square foot per lb. of weight in the Australian crane, which weighs 21 lbs. and yet flies well. If he were to adopt the last or smallest proportion, a man weighing 12 stone would require a pair of wings each of them 14 feet long by 3 feet broad, or double the area of an ordinary room door, to carry him, without taking into account the weight of the wings themselves.

In flying birds there is a strong tripod arrangement to secure firm points of attachment for the wings, and a deep keel in the breast-bone, to which the large pectoral muscles are secured. Think of the wings I have described and the absence of pivots, keel, and muscles in man, and it will be tolerably obvious why he cannot fly, even with artificial wings.

But it might be contended that a man's strength is in his legs rather than in his arms, and that it is conceivable that a successful flying-apparatus might be made if adapted for the most, instead of the least, favourable application of his bodily strength.

According to D. K. Clark ("Rules, Tables, and Data," pp. 719 and 720), a labourer working all day exerts on an average 13 horse-power. The maximum power of a very strong man for a very short time is 46 horse-power.

According to Dr. Houghton ("Animal Mechanics"), the oarsmen in a boat-race of 1 mile, rowed in 7 minutes, exerted each 26 horse-power.

Suppose we take the rowing case as the maximum maintainable for, say, 7 minutes, by a man weighing 168 lbs. Then in flight he would have to sustain a weight of

$$\frac{168}{26} = 64\frac{1}{2} \text{ lbs.}$$

per horse-power exerted, besides the weight of the apparatus.

Now, we shall find later that no birds support even half that weight per horse-power which they have the power to exert, and that recent aeroplane experiments prove its impossibility. On the ground, therefore, that he is too heavy in proportion to his strength, it is clear that man is unfitted for flight, as well as because his limbs are not adapted for it.

It does not follow, however, that by aid of mechanisms apart from his own body, and worked by power independent of his own strength, man may not imitate, compete with, and even outdo the fowls of the air.

Let us consider a few facts showing what birds can do. A gannet hovers in the air above the sea. Suddenly he nearly closes his wings, swoops down, and with a splash disappears below the surface. Shortly after he reappears with a fish in his mouth, which he swallows in a few gulps; then, after swimming on the surface a little, he reascends into the air to repeat the operation.

The swallow rises into the air with a few rapid movements of the wings, then slides down as though on an aerial switch-back, and then up again till he nearly reaches his original height, or he circles round by raising one wing, like a runner rounding a curve.

The condor vulture, which measures sometimes 15 feet across the wings, will fly upwards till quite out of sight.

A flock of cranes have been seen migrating at a height of three miles, and proceeding apparently without any movement of the wings.

The peregrine falcon will swoop down upon a partridge, and, missing it by a doubling movement of the latter, will slide upwards, thus converting his kinetic into new potential energy. He will then turn and descend again, this time securing his prey.

Mr. J. E. Harting, one of the principal British ornithological authorities, has, after careful investigation, arrived at the conclusion that the speed of falcons in full flight is about 60 miles per hour. (*Field*, December 5, 1891, p. 856).

Mr. W. B. Tegetmeier, another well-known authority, gives (*Field*, January 22, 1887, p. 114) the results of a number of experiments on the speed of homing pigeons, made under the auspices of the United Counties Flying Club in 1883. The average speed of the winner in eighteen races was 36 miles, and the maximum 55 miles per hour. The greatest distance flown was 309 miles.

The albatross, the largest web-footed bird, measuring sometimes 17 feet from tip to tip of wing, and weighing up to 20 lbs., frequently accompanies ocean steamers from the Cape to Melbourne, a distance of 5,500 knots, without being seen to rest on the way.

An American naturalist, Mr. J. Lancaster, who spent no less than five years on the west coast of Florida ("Problem of the Soaring Bird," *American Naturalist*, 1885, pp. 1055-1162), in order to study the habits of aquatic and other birds which frequent these shores, arrived at the following conclusions, viz. :—

Though all birds move their wings sometimes, many can remain indefinitely in the air, with wings extended and motionless, and either with or without forward movement. This he calls "soaring."

The wing-area of soaring birds varies from 1 to above 2 square feet per lb. of weight.

The larger the wings per lb. of weight the greater the power to soar.

The heavier the bird the steadier his movements.

Soaring birds always face the wind, which, if they do not move forward or downward, must not blow at a less speed than 2 to 5 miles per hour.

Mr. Lancaster specially watched a flock of buzzards about 30 feet above his head, waiting for him to leave the body of a dead porpoise. Their wings were about 8 feet from tip to tip, and their average weight about 6 lbs. During three hours at mid-day, when the wind which they faced was very strong, they flapped their wings about twenty times each. Later, during two hours, when the wind had subsided, they never moved them at all.

Mr. Lancaster timed frigate birds, and found them able to go at the rate of 100 miles per hour, and that on fixed wings; he is of opinion that at all events up to that speed they can fly just as fast as they please. He says, further, that the same birds can live in the air a week at a time, night and day, without touching a roost, and that buzzards, cranes, and gannets can do the same for several hours at a time.

The observed facts relating to the phenomena of flight are still but very imperfectly understood. That a bird should be able to maintain a downward pressure on the air sufficient to counteract the effect of its own weight, and a backward pressure sufficient to force itself forward at such speeds as I have named, seems wonderful enough when it is known that it continuously operates its wings. But that it should be able to do the same without any muscular movement at all is almost incomprehensible. It seems to be an instance of the suspension of the laws of gravity and of the existence of cause without effect, and of effect without cause. It is not a case of flotation, like a balloon, for any bird falls to the earth like a stone when shot. Mr. Lancaster suggests that the bird's own weight is the force which enables him to counteract the effect thereof, but this explanation is, I confess, beyond my comprehension.

It seems to me that for every pound of his weight pressing downwards there must be an equivalent force pressing upwards. This can be produced only by his giving downward motion to the air previously at rest, or by his arresting previous motion of air in an upward direction. The latter alternative involves the supposition that the air-currents which soaring birds face are not, as Mr. Lancaster believes, always horizontal, but must have, to some extent, an upward direction. If a parachute were falling in a current of air which was moving upwards at the same rate as the parachute fell, it would obviously retain its level, yet gravity would be acting. So, if a bird with extended wings were sliding down a stream of air which was tending upwards at the same angle and same velocity, the phenomenon of soaring would be produced.

Weight of Birds in relation to their Bulk.—It is generally believed that birds are lighter, bulk for bulk, than other animals, and that to this lightness they owe, in some degree, their power of flight and of floating on water. To account for this it is said that their bone-cavities are filled with air, and that some, though not even all, flying birds have small air-sacs under the skin. It

is clear, however, that displacement of external air by air-filled cavities can only assist aerial floatation to an infinitesimal extent, unless highly heated. Such cavities would, however, help aquatic birds to swim, if situated under the immersed portion of their bodies, which is not always the case.

Some aquatic birds, such as swans, swim with head, neck, wings, tail, and half their bodies out of the water. The specific gravity of fishes and land animals is clearly about the same as water. For, when swimming, they can keep only a small portion of their heads above the surface, and that by continued exertion. Are, then, birds, in the substance of their bodies, less dense than other animals, although also composed of flesh, blood, and bone, and these components in similar proportions and of similar character and texture? If they are, then land animals might have been made lighter in proportion to their bulk or smaller in proportion to their weight than they have been. If they are not, how is it that some of them can swim and float high out of the water?

Having an opportunity recently of inspecting a large wild, or whooper, swan, I ascertained its weight to be 14 lbs. I noticed that the whole of the under-part of the body, which would be immersed when swimming, was covered with feathers and underlined with down to an average depth of not less than $1\frac{1}{2}$ inches, or, when closely pressed, say $1\frac{1}{4}$ inches. The immersed surface I estimated at $1\frac{1}{2}$ square feet. The weight of water displaced by this feather and down jacket, and the consequent extra buoyancy produced thereby was no less than 9.78 lbs. This would account for two-thirds of the bird's body being out of water when swimming, even if the body were of the same specific gravity as water.

I next procured a freshly-shot wild duck, which weighed $2\frac{1}{2}$ lbs., and placed it in a tank of sea-water. It floated. I found the area of its immersed surface to be 54 square inches, and the average depth of its under-feathers and down to be $\frac{3}{4}$ inch. The water displaced by this envelope would weigh 1.5 lbs., and would support three-fifths of its entire weight. I then had it denuded of all its feathers and down, and again placed in the tank. It then slowly sank to the bottom.

These experiments, so far as they go, seem to prove conclusively that birds are not lighter, bulk for bulk, than other animals, but, on the other hand, about the same specific gravity, and that their floating power lies entirely in the thick jacket or life-belt with which nature has furnished those, and those only which are intended to swim.

Inasmuch, therefore, as the specific gravity of the actual bodies of all animals appears to be about the same, there is no reason to believe that any could have been constructed of lighter material or to lighter design.

Weight in Relation to their Energy.—But notwithstanding this uniformity of specific gravity, there remains the curious fact that flying birds can exert continuously about three times the horse-power per lb. of weight that man can—and, indeed, about three times what is possible for the horse. This marvellous flow of energy in proportion to weight is probably due to rapidity of limb-action rather than to increase of muscular stress. I have timed sea-gulls and found them to flap their wings two hundred times per minute when flying at about 24 knots per hour, and have estimated eider-ducks, making about 36 knots per hour, to be flapping their wings five hundred times in a minute. I say "estimated," for their movements are too rapid for precise counting. This outpouring of energy, which seems to me to be unequalled in terrestrial animals, is nevertheless maintained by birds for indefinitely long periods of time.

A proportionately increased rate of combustion and renovation of tissue as well as of food-consumption are necessary consequences. The higher temperature of the bodies of birds, as compared with other animals,¹ and the well-known voracity of those which, like sea-birds, are almost continuously on the wing, are circumstances which seem to point to the same conclusion. It is confirmed by what we know of steam and other motors. For instance, if a steamship were so built and proportioned that a ton of coal per hour consumed in the boilers would maintain the pressure at 100 lbs. per square inch and produce 1000 horse-power at the propeller; and then if, without other alteration, firing was slackened until the steam fell to 50 lbs. per square inch and there maintained, it is clear that the horse-power produced would be greatly lessened, and so would the temperature

of the steam in the boilers, steam-pipes, and cylinders. Thus, other things being equal, the temperature of the steam would rise and fall with the energy given forth by the mechanism.

The suggestion is that the higher temperature of birds, as compared with other animals, is similarly connected with their superior power of producing and maintaining energetic effort.

Aerial Navigation.

Let us now consider what man has done, and may be able to do, in aerial navigation by aid of contrivances which, as in the case of railway locomotives and ocean steamers, are propelled by a power other than that of his own body.

The scientific world is greatly indebted to Mr. Hiram S. Maxim, of London, for recording, in a clear and readable form, the present position of aeronautic mechanisms.¹ So far, the only contrivances which have been fairly successful are balloons, which, unlike birds, depend on atmospheric displacement for their power of sustaining weight or rising or falling.

In balloon experiments our French neighbours have led the way, from the first attempt of the Montgolfier brothers in 1783. During the last twenty years they have made numerous experiments and substantial improvements. Captain Renard and other officers of the French army have constructed a fish-shaped apparatus, and inflated it with hydrogen. It is driven by an electric motor of $8\frac{1}{2}$ horse-power, and has sufficient buoyancy to carry two aeronauts and all necessary accessories. In fair weather Captain Renard has succeeded in travelling at the rate of $12\frac{1}{2}$ miles per hour, in steering in any direction, and even in returning to his point of departure. The balloon, it is said, always keeps level, and so far there have not been any accidents; but no expedition has been attempted in wet or windy weather.

Except that a more powerful motor, going at a higher speed, might be fitted to such an apparatus, Mr. Maxim thinks that it is as near perfection as is ever likely to be reached by a machine depending on aerial floatation. He proceeds to give an account of some experiments made by Prof. S. P. Langley, of the Smithsonian Institution, Washington, and of others by himself, to a certain how much power is required to produce artificial flight by means of aero-planes, after the manner of birds, and whether such power can be obtained without exceeding the weight which it would itself sustain.

He says that heavy birds, with relatively small wings, carry about 150 lbs. per horse-power exerted, and birds such as the albatross and vulture probably about 250 lbs. Prof. Langley, with small slanting planes, was able to carry 250 lbs. per horse-power exerted; and Mr. Maxim, using heavier weights in proportion to plane-area, 133 lbs. per horse-power, and using lighter ones, nearly the same as Prof. Langley.

Mr. Maxim has lately developed his energies to constructing a motor which should meet the requirements of the case, and has succeeded, he says, in producing one: a steam-engine burning naphtha and with atmospheric condenser, with a total weight of 8 lbs. per horse-power. He thinks, however (*Engineer*, January 13, 1893, p. 28), that by using light naphtha and its vapour in the boiler instead of water, as well as in the furnace as fuel, a weight as low as 5 lbs. per horse-power may be reached.

Meanwhile Prof. Langley's ideas have been embodied in an experimental flying-machine, a drawing and description of which will be found in the *Daily Graphic* for July 1, 1893. The body, which resembles that of a bird and is 15 feet long, contains the propelling machinery in duplicate. The wings, which are 40 feet across, are of China silk spread on a tubular framework, stiffened with wire trusses. The boilers use liquid fuel and contain a highly volatile fluid. The capabilities of the machine have not yet been practically tested.

Promising as are the results hitherto obtained, they are as yet far from placing us on a level with birds in power to utilise the atmosphere as a navigating medium. The absolutely necessary power of delicate guiding, in rising, falling, and turning, whatever the direction or force of the wind, has yet to be considered and worked out. What would happen in case of a temporary breakdown of the aero-plane machinery we shudder to think of.

An important step has been effected by the discovery that parachutes with tubular orifices at the top are comparatively safe appliances for descending to the earth from indefinitely high altitudes. Perhaps it may be arranged that each aeronaut should

¹ *Chambers' Encyclopædia*, "Bird and Animal Heat"; *Lehrbuch der Zoologie*, by Prof. Hertwig, p. 538.

¹ "Progress in Aerial Navigation," by Hiram S. Maxim, *Fortnightly Review*, October, 1892.

be able, at a moment's warning, to gird himself with one of these as with a life-belt on board ship, and so descend in safety, or one or more automatically opening in case of disaster might be fitted to the *aéro-plane* as a whole.

Eventual Exhaustion of Fuel Supply.

I have still to refer to one other question, the consideration of which must always give rise to very serious thoughts. We have seen that the decisive victories which, in modern times, man has gained over matter and over other animals have been due to his use of power derived from other than animal sources. That power has invariably proceeded from the combustion and the destruction of fuel, the accumulations of which in the earth are necessarily limited.

Mechanical appliances, involving the consumption of fuel, have, for a century at least, been multiplying with alarming rapidity. Our minds have been set mainly on enlarging the uses and conveniences of man, and scarcely at all on economising the great sources of power in nature, which are now for the most part its fuels. Terrible waste of these valuable stores is daily going on in almost every department of use. Once exhausted they can never be replaced. They have been drawn upon to some extent for 1000 years, and extensively for more than 100. Authorities say that another 1000 years will exhaust all the more accessible supplies. But suppose they last 5000 years—what then? Why, then, as far as we can at present see, our only motive-powers will be wind and water and animals, and our only mode of transit, sailing and rowing, driving, cycling, riding, and walking.

Sir Robert Ball has estimated that in not less than 5,000,000 and not more than 10,000,000 years the sun will have become too cold to support life of any kind on this planet. Between the 5000 years when fuel will certainly be exhausted and the 5,000,000 years when all life may be extinguished, there will still be 4,995,000 years when, according to present appearances, man will have to give up his hard-earned victories over matter and other animals, and the latter will again surpass him, each in its own element, because he has no fuel.

Conclusion.

Leaving to our posterity these more remote troubles, we may, I think, justly draw from the entire discussion the conclusion that we have still a great deal to learn from mechanisms as they exist in nature. Great as have been the achievements of man since he first began to study mechanical science, with a view to directing the great sources of power in nature for his own use and convenience, the entire field of research is by no means yet fully exhausted. We must continue to study the same science with undiminished ardour. In so doing we shall do well to bear in mind that success can be achieved only by the patient, accurate, and conscientious observation of the great facts of nature, which are equally open to us all and waiting for our attention; and by drawing correct inferences therefrom, and by applying such inferences correctly to the fulfilment of the future needs and destiny of our race.

SECTION H.

ANTHROPOLOGY.

OPENING ADDRESS BY ROBERT MUNRO, M.A., M.D.,
F.R.S.E., PRESIDENT OF THE SECTION.

The science of anthropology, in its widest sense, embraces all the materials bearing on the origin and history of mankind. These materials are so comprehensive and diversified, both in their character and methods of study, that they become necessarily grouped into a number of subordinate departments. From a bird's-eye point of view, however, one marked line of demarcation separates them into two great divisions, according as they relate to the structure and functions of man's body, or to the works he has produced—a classification well defined by the words *anthropology* and *archæology*. The former, in its limited acceptance, deals more particularly with the development of man—his physical peculiarities, racial distinctions, linguistic manifestations, mental endowments, and, in short, every morphological or mental modification he has undergone amidst the ever-changing phenomena of his environments. The latter, on the other hand, takes cognisance of man merely as a handicraftsman. During his long journey in past time he has left behind him, scattered on the

highways and byways of primeval life, numerous traces of his ways, his works, his culture, and his civilisation, all of which fall to be collected, sorted, and interpreted by the skilled archæologist. In their general aspects and relationship to each other most of the leading subjects in both these branches of the science have already been expounded, in the presidential addresses of my predecessors, by men so distinguished in their respective departments that they have left little to be said by anyone who attempts to follow in their footsteps. There is, however, one phase in the progressive career of man which has not hitherto been so fully illustrated as the subject appears to me to merit. I refer to the direct and collateral advantages which the erect position has conferred on him; and to this I will now briefly direct your attention, concentrating my observations successively on the following propositions:—

(1) The mechanical and physical advantages of the erect position.

(2) The differentiation of the limbs into hands and feet.

(3) The relation between the more perfect condition of these organs and the development of the brain.

In the process of organic evolution it would almost appear as if nature acted on teleological principles, because many of her products exhibit structures which combine the most perfect adaptation of means to ends along with the greatest economy of materials. This is well exemplified in some of the structural details of the organs of locomotion in which many of the so-called mechanical powers may be seen in actual use. The primary object of locomotion was to enable the organism to seek its food over a larger area than was attainable by a fixed position. The acquisition of this power was manifestly so advantageous to animal life that the principles by which it could be effected became important factors in natural selection. I need not here dwell on the various methods by which this has been accomplished in the lower forms of life, but proceed at once to point out that in the higher vertebrates the problem resolved itself into the well-known mechanism of four movable limbs, capable of supporting and transporting the animal. As these quadrupedal animals became more highly differentiated, in virtue of the necessities of the struggle for life and the different and ever-varying conditions of their surroundings, it followed that the limbs became also modified so as to make them suitable, not only for locomotion in various circumstances, but also useful to the animal economy in other ways. Hence they were subjected to an endless variety of secondary influences, which finally adapted them for such diverse purposes as swimming, flying, climbing, grasping, &c. The anterior limbs, owing to their proximity to the head, were more frequently selected for such transformations as may be seen, for instance, in the wings of a bird. But whatever modifications the fore limbs may have undergone, no animal, with the exception of man, has ever succeeded in divesting them altogether of their primary function. This exceptional result was due to the erect position, which necessitated a complete division of labour as regards the functions of the limbs—the two anterior being entirely restricted to manipulative and prehensile purposes, and the two posterior exclusively devoted to locomotion. Coincident with this notable specialisation of their function a new field for advancement was opened up to man, in which intelligence and mechanical skill became the leading factors in his further development.

Man is thus distinguished from all other animals by the fact that, in the normal position of walking or running, he carries his body upright, *i.e.* with the axis of the vertebral column perpendicular, instead of horizontal or oblique. In this position all its parts are so arranged as to require a minimum amount of exertion in the performance of their functions. If any of the other higher vertebrates should ever assume an erect attitude it can only be maintained temporarily, and its maintenance involves an additional expenditure of force. In a certain sense a bird may be looked upon as a biped, but there is this distinction to be drawn between it and man, *viz.* that the former has not only its body balanced obliquely on its two legs, but also its fore limbs converted into special organs for motion in the air. The anthropoid apes hold an intermediate position, and so carry their body in a semi-erect attitude. But this shortcoming in reaching the perfectly upright position, however slight it may be in some of these animals, represents a wide gap which can only be fully appreciated by a careful study of the physiological and psychological phenomena manifested in their respective life-functions.

Everyone acquainted with the ordinary operations of daily life knows how much labour can be saved by attention to the mere mechanical principles involved in their execution. In carrying a heavy load the great object is to adjust it so that its centre of gravity comes as nearly as possible to the vertical axis of the body, as otherwise force is uselessly expended in the effort to keep the entire moving mass in stable equilibrium—a principle well exemplified by the Italian peasant girl when she poises her basket of oranges on her head. Once upon a time a powerful waterman, accustomed to use buckets double the size of those of his fellow-watermen, had the misfortune to have one of them broken. As he could not, then and there, get another bucket to match the remaining one, and wishing to make the best possible use of the appliances at hand, he replaced the broken vessel by one half its size. He then filled both with water and attempted to carry them, as formerly, attached to a yoke, one on each side of him. But to his astonishment this arrangement would not work. The yoke became uneven, and the effort to keep it balanced on his shoulders was so troublesome that he could not proceed. This emergency led to serious reflection, but, after some experimental trials, he ascertained that, by merely making the arm of the yoke on which the small bucket was suspended double the length of the other, he could carry both buckets without inconvenience.

But let me take one other illustration. Suppose that two burglars have concocted a plan to rob a richly-stored mansion by getting access to its rooms through the windows of an upper story. In order to carry out this design they secure a ladder, easily transported by the two together though too heavy for one. So, bearing the ladder between them one at each end, they come to the house. After a considerable amount of exertion they succeed in placing the ladder in an upright position against the wall, and then one of the men mounts its steps and enters the house. The man left outside soon realised that, once the ladder was balanced perpendicularly, he himself could then easily control it. Moreover, he made the discovery that by resting its weight on each leg alternately, he could gradually shift its position from one window to another. Thus there was no interruption or limit to the extent of their depredations. Experience quickened their perceptions, and ultimately they became adepts in their respective departments—the one in the art of moving the ladder, and the other in the science of the nimble-fingered gentry. The division of labour thus practised by these two men accurately represents what the attainment of the erect attitude has accomplished for man by setting free his upper limbs from any further participation in the locomotion of his body.

The continued maintenance of this unique position necessitated great changes in the general structure of the body. The solution of the problem involved the turning of the ordinary quadruped a quarter of a circle in the vertical plane, thus placing the axis of the spine perpendicular, and consequently in line with the direction of the posterior limbs; and to effect this the osseous walls of the pelvis underwent certain modifications, so as to bear the additional strain put upon them. Stability was given to the trunk in its new position by the development of special groups of muscles, whose powerful and combined actions render to the movements of the human body their characteristic freedom and gracefulness. The lower limbs were placed as widely apart as possible at their juncture with the pelvis, and the thigh- and leg-bones were lengthened and strengthened so as to be capable of supporting the entire weight of the body and of transporting it with due efficiency when required. The spinal column assumed its well-known curves, and the skull, which formerly had to be supported by a powerful muscle attached to the spinous processes of the cervical vertebrae (*ligamentum nuchæ*), moved backwards until it became nearly equipoised on the top of the vertebral column. The upper limbs, instead of taking part in their original function of locomotion, were now themselves carried as flail-like appendages, in order to give them as much freedom and range of action as possible. The shoulder-blades receded to the posterior aspect of the trunk, having their axes at right-angles to that of the spine. Further, like the haunch-bones, they underwent certain modifications, so as to afford points of attachment to the muscles required in the complex movements of the arms. In the pendulous position each arm has its axis at right angles to that of the shoulder, but by a common muscular effort the two axes can be readily brought into line. The elbow-joint became capable of performing the movements of complete extension,

flexion, pronation, and supination—in which respects the upper limb of man is differentiated from that of all other vertebrates.

But it is in the distal extremities of the limbs that the most remarkable anatomical changes have to be noted. The foot is virtually a tripod, the heel and the ball of the great toe being the terminal ends of an arch, while the four outer digital columns group themselves together to form the third, or steadying, point. The outer toes thus play but a subordinate part in locomotion, and, as their prehensile function is no longer of use, they may be said to be fast approaching to the condition of rudimentary organs. The three osseous prominences which form this tripod are each covered with a soft elastic pad, which both facilitates progression and acts as a buffer for deadening any possible shock which might arise in the course of running or leaping. The chief movement in the act of progression is performed by an enormously developed group of muscles known as the calf of the leg, so characteristic of man. The walker is thereby enabled to use the heel and the ball of the great toe as successive fulcrums from which the forward spring is made, the action being greatly facilitated by that of the trunk muscles in simultaneously bending the body forwards. The human foot is thus admirably adapted to be both a pillar for supporting the weight of the body, and a lever for mechanically impelling it forwards. Hence the amount of energy expended in progression is reduced to a minimum, and when estimated proportionally to the size of the body it is believed to be considerably less than that requisite for the corresponding act in quadrupeds.

The anatomical changes effected in the extremity of the upper limb are equally radical, but of a totally different character and scope. Here we have to contemplate the transformation of the same homologous parts into an apparatus for performing a series of prehensile actions of the most intricate character, but among which neither locomotion nor support of the body forms any part whatever. This apparatus is the human hand, the most complete and perfect mechanical organ nature has yet produced. The fingers have become highly developed, and can be opposed singly or in groups to the thumb, so as to form a hook, a clasp, or a pair of pincers; and the palm can be made into a cup-shaped hollow, capable of grasping a sphere. Nor is there any limit to the direction in which many of these manipulations can be performed without any movement of the rest of the body. For example, a pencil held by the thumb and the two forefingers, as in the act of writing, can be placed in all the directions of space by a mere act of volition.

The position of such a perfect piece of mechanism at the extremity of a movable arm attached to the upper part of the trunk, gives to man a superiority in attack and defence over all other animals, on the same principle as a soldier finds it advantageous to fight from higher ground. Moreover, he possesses the power to perform a variety of quick movements, and to assume attitudes and positions eminently adapted for the exercise of that manipulative skill with which he counteracts superior brute force of many of his antagonists. He can readily balance his body on one or both legs, can turn on his heels as if they were pivots, and can prostrate himself comfortably in the prone or supine positions. As the centre of gravity of the whole body is nearly in line with the spinal axis, stable equilibrium is easily maintained by the lumbar muscles. Altogether we have in his physical constitution a combination of structures and functions sufficiently unique in its *tout ensemble* to place man in a category by himself. But at the same time we must not forget that all his morphological peculiarities have been brought about without the destruction of any of the primary and typical homologies common to all the higher vertebrates.

Turning now to the brain, the undoubted organ of the mind, we find in its intellectual and psychical manifestations, a class of phenomena which gives to man's life-functions their most remarkable character. However difficult it may be for our limited understanding to comprehend the nature of conscious sensation, we are forced to the conclusion that the act invariably takes place through the instrumentality of a few nerve-cells, whose functional activity requires to be renovated in precisely the same manner as the muscular force expended in walking. The aggregation of such cells into ganglia and nerves, by means of which reflex action, consciousness, and a variety of psychical phenomena take place, is found to permeate, in a greater or less degree, the whole of the organic world. In the higher vertebrates the seat of these manifestations is almost exclusively confined to an enormous collection of brain substance

placed at the upper end of the vertebral column, and encased in a complete osseous covering called the skull. We learn from numerous experimental researches, carried out by physiologists in recent years, that the brain is a dual organ, consisting of a double series of distinct ganglia and connected to some extent by a complex system of nervous tissues, not only with each other, but with the central seat of consciousness and volition. But the difficulty of determining the nature of its functions, and the *modus operandi* of its psychological manifestations, is so great that I must pass over this part of the subject very lightly indeed. The conditions of ordinary reflex-action require that a group of muscles, by means of which a particular bodily movement is effected, shall be connected with its co-ordinating ganglion by an afferent and an efferent system of nerves. Impressions from without are conveyed by the former, or sensory nerves, to the central ganglion, from which an impulse is retransmitted by the motor nerves and sets in operation the muscular force for producing the required movement. But this efferent message is, in many cases, absolutely controlled by volition, and not only can it prevent the muscular action from taking place, but it can effect a similar movement, *de novo*, without the direct intervention of external impressions at all. Now it has been proved experimentally that the volitional stimulus, which regulates the various movements of the body, starts from definite portions of the brain according to the different results to be produced. This localisation of brain functions, though still far from being thoroughly understood, comes very appropriately into use in this inquiry. From it we learn that the homology which characterises the structural elements of the bodies of animals extends also to the component parts of their respective brains. The law which differentiates animals according to the greater specialisation of the functions of their various organs has therefore its counterpart in the brain, and we naturally expect an increase of brain substance in every case in which the functional activity of a specific organ is extended. Thus the act of stitching with a needle and thread, an act beyond the mental and physical capacity of any animal but man, would entail a certain increase of brain substance, simply in obedience to the great complexity of the movements involved in its execution, over and above that which may be supposed to be due to the intellectual and reasoning faculties which invented it.

That man's brain and his intelligence are correlated to each other is a fact too axiomatic to require any demonstration; nor can it be doubted that the relationship between them is of the nature of cause and effect. But to maintain that the amount of the latter is directly proportional to the size of the former is rather straining the laws of legitimate inference. In drawing any general conclusion of this nature from the bulk of brain substance, there are some modifying influences which cannot be disregarded, such, for example, as the amount of cranial circulation and the quality of the brain cells. But the determination of this point is not the exact problem with which the evolutionist is primarily concerned. To him the real crux in the inquiry is to account for the evolution of man's comparatively large brain under the influence of existing cosmic forces. After duly considering this problem, and casting about for a possible explanation, I have come to the conclusion that not only is it the result of natural laws, but that one of the main factors in its production was the conversion of the upper limbs into true hands. From the first moment that man recognised the advantage of using a club or a stone in attacking his prey or defending himself from his enemies, the direct incentives to a higher brain development came into existence. He would soon learn by experience that a particular form of club or stone was more suitable for his purposes; and if the desiderated object were not to be found among the natural materials around him, he would proceed to manufacture it. Certain kinds of stones would be readily recognised as better adapted for cutting purposes than others, and he would select his materials accordingly. If these were to be found only in a special locality, he would visit that locality whenever the prized material was needed. Nor would it be an unwarrantable stretch of imagination to suppose that the circumstances would lead him to lay up a store for future use. These simple acts of intelligence assume little more than may be seen in the actions of many of the lower animals. Consciousness of his power to make and to wield a weapon was a new departure in the career of man, and every repetition of such acts became an effective and ever-accumulating training force. What a memorable event in the history of humanity was the manufacture of the first sharp stone implement! Our sapient ancestor,

who first used a spear tipped with a sharp flint, became possessed of an irresistible power over his fellow men. The invention of the bow and arrow may be paralleled with the discovery of gunpowder and the use of cannon, both of which revolutionised the principles of warfare in their respective ages. The art of making fire had a greater influence on human civilisation than the modern discovery of electricity. The first boat was in all probability a log—an idea which might have been suggested by the sight of an animal clinging to a floating piece of wood carried away by a flood. To scoop this log into a hollow boat was an afterthought. The successive increments of knowledge by which a single-tree canoe has been transformed into a first-class Atlantic liner are scattered through the unwritten and written annals of many ages. In his expeditions for hunting, fishing, fruit-gathering, &c., primitive man's acquaintance with the mechanical powers of nature would be gradually extended, and *pari passu* with the increasing range of his knowledge there would be a corresponding development in his reasoning faculties. Natural phenomena suggested reflections as to their causes and effects, and so by degrees they were brought into the category of law and order. Particular sounds would be used to represent specific objects, and these would become the first rudiments of language. Thus each generalisation when added to his previous little stock of knowledge widened the basis of his intellectual powers, and as the process progressed man would acquire some notion of the abstract ideas of space, time, motion, force, number, &c.; and continuous thought and reasoning would ultimately become habitual to him. All these mental operations could only take place through the medium of additional nerve cells, and hence the brain gradually became more bulky and more complex in its structure. Thus the functions of the hand and of the brain have been correlated in a most remarkable manner. Whether the mechanical skill of the hand preceded the greater intelligence of the brain, or *vice versa*, I will not pretend to say. But between the two there must have been a constant interchange of gifts. According to Sir C. Bell, "the hand supplies all instruments, and by its correspondence with the intellect gives him universal dominion." ("The Hand," &c. "Bridgewater Treatise," p. 38.)

That mind, in its higher psychical manifestations, has sometimes been looked upon as a spiritual essence which can exist separately from its material basis need not be wondered at when we consider how the pleasing abstractions of the poet, or the fascinating creations of the novelist, roll out, as it were, from a hidden cavern without the slightest symptom of physical action. It is this marvellous power of gathering and combining ideas, previously derived through the ordinary senses, which gives a *primâ facie* appearance of having here to deal with a force exterior to the brain itself. But indeed it is questionable if such psychological phenomena are really represented by special organic equivalents. May they not be due rather to the power of volitional reflection which summons them from the materials stored up by the various localised portions into which the brain is divided? From this point of view there may be many phases of pure cerebration which, though not the result of direct natural selection, have nevertheless as natural and physical an origin as conscious sensation. Hence imagination, conception, idealisation, the moral faculties, &c., may be compared to parasites which live at the expense of their neighbours. After all the greatest mystery of life lies in the simple acts of conscious sensation, and not in the higher mental combinations into which they enter. The highest products of intellectuality are nothing more than the transformation of previously existing energy, and it is the power to utilise it that alone finds its special organic equivalent in the brain.

But this brings us on controversial ground of the highest importance. Prof. Huxley thus expresses his views on the phase of the argument now at issue:—

"I have endeavoured to show that no absolute structural line of demarcation, wider than that between the animals which immediately succeed us in the scale, can be drawn between the animal world and ourselves; and I may add the expression of my belief that the attempt to draw psychical distinction is equally futile, and that even the highest faculties of feeling and of intellect begin to germinate in lower forms of life." ("Evidences as to Man's Place in Nature," p. 109.)

On the other hand, Mr. Alfred R. Wallace, who holds such a distinguished position in this special field of research, has promulgated a most remarkable theory. This careful investigator, an original discoverer of the laws of natural selection, and

a powerful advocate of their adequacy to bring about the evolution of the entire organic world, even including man up to a certain stage, believes that the cosmic forces are insufficient to account for the development of man in his civilised capacity. "Natural selection," he writes, "could only have endowed savage man with a brain a few degrees superior to that of an ape, whereas he actually possesses one very little inferior to that of a philosopher." This deficiency in the organic forces of nature he essays to supply by calling in the guiding influence of a "superior intelligence." In defending this hypothesis from hostile criticism he explains that by "superior intelligence" he means some intelligence higher than the "modern cultivated mind," something intermediate between it and Deity. But as this is a pure supposition, unsupported by any evidence, and merely a matter of personal belief, it is unnecessary to discuss it further. I would just, *en passant*, ask Mr. Wallace why he dispenses with this "higher intelligence" in the early stages of man's evolution, and finds its assistance only requisite to give the final touches to humanity.

In dealing with the detailed objections raised by Mr. Wallace against the theory of natural selection as applied to man, we are, however, strictly within the sphere of legitimate argument; and evolutionists are fairly called upon to meet them. As his own theory is founded on the supposed failure of natural selection to explain certain specified peculiarities in the life of man, it is clear that if these difficulties can be removed, *causit questio*. It is only one of his objections, however, that comes within the scope of my present inquiry, viz. that which is founded on the supposed "surplusage" of brain power in savage and prehistoric races.

In comparing the brains of the anthropoid apes and man Mr. Wallace adopts the following numbers to represent their proportional average capacities, viz. anthropoid apes 10, savages 26, and civilised man 32—numbers to which there can be no objection, as they are based on data sufficiently accurate for the requirements of this discussion. In commenting on the mental ability displayed in actual life by the recipients of these various brains, he states that savage man has "in an undeveloped state faculties which he never requires to use," and that his brain is much beyond his actual requirements in daily life. He concludes his argument thus:—"We see, then, that whether we compare the savage with the higher developments of man, or with the brutes around him, we are alike driven to the conclusion that in his large and well-developed brain he possesses an organ quite disproportionate to his actual requirements—an organ that seems prepared in advance, only to be fully utilised as he progresses in civilisation. A brain one half larger than that of the gorilla would, according to the evidence before us, fully have sufficed for the limited mental development of the savage; and we must therefore admit that the large brain he actually possesses could never have been solely developed by any of those laws of evolution whose essence is that they lead to a degree of organisation exactly proportionate to the wants of each species, never beyond those wants; that no preparation can be made for the future development of the race; that one part of the body can never increase in size or complexity, except in strict co-ordination to the pressing wants of the whole. The brain of prehistoric and of savage man seems to me to prove the existence of some power distinct from that which has guided the development of the lower animals through their ever-varying forms of being." ("Natural Selection," &c. 1891, p. 193.)

With regard to the closing sentence of the above quotation, let me observe that the cosmic forces, under which the lower animals have been produced by means of natural selection, do not disclose, either in their individual or collective capacity, any guiding power in the sense of a sentient influence, and I believe that the "distinct power" which the author summons to his aid, apparently from the "vast deep," to account for the higher development of humanity is nothing more than the gradually acquired product of the reasoning faculties themselves. Not that, for this reason, it is to be reckoned less genuine and less powerful in its operations than if it had emanated from an outside source. The reasoning power displayed by man is virtually a higher intelligence, and, ever since its appearance on the field of organic life, it has, to a certain extent, superseded the laws of natural selection. Physical science has made us acquainted with the fact that two or three simple bodies will sometimes combine chemically so as to produce a new substance, having properties totally different from

those of either constituents in a state of disunion. Something analogous to this has taken place in the development of man's capacity for reasoning by induction. Its primary elements, which are also those of natural selection, are consciousness, sensation, heredity, and a few other properties of organic matter, elements which are common, in a more or less degree, to all living things. As soon as the sequence of natural phenomena attracted the attention of man, and his intelligence reached the stage of consecutive reasoning on the invariableness of certain effects from given causes, this new power came into existence; and its operations are, apparently, so different from those of its component elements that they can hardly be recognised as the off-spring of natural forces at all. Its application to the adjustment of his physical environments has ever since been one of the most powerful factors, not only in the development of humanity, but in altering the conditions and life-functions of many members of the animal and vegetable kingdoms.

I have already pointed out that the brain can no longer be regarded as a single organ, but rather as a series of organs connected by bonds of union—like so many departments in a Government office in telephonic communication—all, however, performing special and separate functions. When, therefore, we attempt to compare the brain capacity of one animal with that of another, with the view of ascertaining the quality of their respective mental manifestations, we must first determine what are the exact homologous parts that are comparable. To draw any such inference from a comparison of two brains, by simply weighing or measuring the whole mass of each, would be manifestly of no scientific value. For example, in the brain of a savage the portion representing highly skilled motor energies might be very much larger, while the portion representing logical power might be smaller, than the corresponding parts in the brain of a philosopher. But should these inequalities of development be such as to balance each other, the weight of the two organs would be equal. In this case what could be the value of any inference as to the character of their mental endowments? Equal-sized brains do not display equivalent, nor indeed analogous, results. To postulate such a doctrine would be as irrational as to maintain that the walking capacities of different persons are directly proportional to the weight of their bodies. Similar remarks are equally applicable to the skulls of prehistoric races, as it would appear that evolution had done the major part of its work in brain development long before the days of neolithic civilisation. Huxley's well-known description of the Engis skull—"a fair average skull, which might have belonged to a philosopher, or might have contained the thoughtless brains of a savage"—goes far to settle the question from its anatomical point of view. Until localisation of brain functions makes greater progress it is, therefore, futile to speculate to any great extent on the relative sizes of the skulls of different races either in present or prehistoric times.

But there is another aspect of the question which militates against Mr. Wallace's hypothesis, viz. the probability that many of the present tribes of savages are, in point of civilisation, in a more degenerate condition than their forefathers who acquired originally higher mental qualities under natural selection. There must surely be some foundation of truth in the widely-spread tradition of the fall of man. And, if such be the case, we naturally expect to find some stray races with inherited brains of greater capacity than their needs, in more degenerate circumstances, may require. An exact equivalent to this may be seen in the feeble intellectuality of many of the peasants and lower classes among the civilised nations of modern times. Yet a youth born of such parents, if educated, often becomes a distinguished philosopher. It is well known that if an organ ceases to perform its functional work it has a tendency to deteriorate and ultimately to disappear altogether. But from experience we know that it takes a long time for the effects of disuse to become manifest. It is this persistency that accounts for a number of rudimentary organs, still to be met with in the human body, whose functional activity could only have been exercised ages before man became differentiated from the lower animals. Such facts give some support to the suggestion, previously made, that philosophy, as such, has no specially localised portion in the brain. Its function is merely to direct the current of mental forces already existing.

But, again, Mr. Wallace's argument involves the assumption that the unnecessarily large brain of the savage had been constructed on teleological principles for the sole purpose of philosophising. My opinion is that the greater portion of this so-called surplusage is

the organic representative of the energy expended in the exercise of the enormous complexity of human actions, as displayed in the movements of his body and in the skilful manipulations necessary to the manufacture of implements, weapons, clothing, &c. All such actions have to be represented by a larger bulk of brain matter than is required for the most profound philosophical speculations. The kind of intelligence evinced by savages, however low their position in the scale of civilisation may be, is different from, and incomparably greater than, that manifested by the most advanced of the lower animals. To me it is much more rational to suppose that the development of the large brain of man corresponded, *pari passu*, with that of his characteristic physical attributes, more especially those consequent on the attainment of the upright position. That these attributes were acquired exclusively through the instrumentality of the cosmic forces was, as the following quotation will show, the opinion of Mr. Darwin:—"We must remember that nearly all the other and more important differences between man and quadrumana are manifestly adaptive in their nature, and relate chiefly to the erect position of man; such as the structure of his hand, foot, and pelvis, the curvature of his spine, and the position of his head." ("Descent of Man," p. 149.) Mr. Wallace, however, considers the feet and hands of man "as difficulties on the theory of natural selection." "How," he exclaims, "can we conceive that early man, as an animal, gained anything by purely erect locomotion? Again, the hand of man contains latent capacities and powers which are unused by savages, and must have been even less used by palæolithic man and his still ruder predecessors. It has all the appearance of an organ prepared for the use of civilised man, and one which was required to render civilisation possible." ("Natural Selection," p. 198.) But here again this acute observer diverges into his favourite by-path, and introduces a "higher intelligence" to bridge over his difficulties.

We have now reached a stage in this inquiry when a number of questions of a more or less speculative character fall to be considered. On the supposition that, at the start, the evolution of the hand of man was synchronous with the higher development of his reasoning faculties, it is but natural to ask where, when, and in what precise circumstances this remarkable coalition took place. I would not, however, be justified in taking up your time now in discussing these questions in detail; not because I think the materials for their solution are unattainable, but because, in the present state of our knowledge, they are too conjectural to be of scientific value. In the dim retrospective vista which veils these materials from our cognisance I can only see a few faint landmarks. All the osseous remains of man which have hitherto been collected and examined point to the fact that, during the larger portion of the quaternary period, if not, indeed, from its very commencement, he had already acquired his human characteristics. This generalisation at once throws us back to the tertiary period in our search for man's early appearance in Europe. Another fact—disclosed by an analysis of his present corporeal structure—is that, during a certain phase of his previous existence, he passed through a stage when his limbs, like those of the present anthropoid apes, were adapted for an arboreal life. We have therefore to look for the causes which brought about the separation of man from his quadrumanous congeners, and entailed on him such a transformation in his form and habits, in the physical conditions that would supervene on a change from a warm to a cold climate. In the gradual lowering of the temperature of the subtropical climate which prevailed in Central Europe and the corresponding parts of Asia during the Miocene and Pliocene periods, and which culminated in the great Ice age, together with the concurrent changes in the distribution of land, seas, and mountains, we have the most probable explanation of these causes. Whether man forsook his arboreal habits and took to the plains from overcrowding of his own species in search of different kinds of food, before this cold period subjected him to its intensely adverse circumstances, it would be idle for me to offer an opinion. Equally conjectural would it be to inquire into the exact circumstances which led him to depend exclusively on his posterior limbs for locomotion.

During this early and transitional period in man's career there was no room for ethics. Might was right, whether it emanated from the strength of the arm, the skill of the hand, or the cunning of the brain. Life and death combats would decide the fate of many competing races. The weak would succumb to

the strong, and ultimately there would survive only such as could hold their own by flight, strength, agility, or skill, just as we find among the races of man at the present day.

In summing up these somewhat discursive observations, let me just emphasise the main points of the argument. With the attainment of the erect position, and the consequent specialisation of his limbs into hands and feet, man entered on a new phase of existence. With the advantage of manipulative organs and a progressive brain he became *Homo sapiens*, and gradually developed a capacity to understand and utilise the forces of nature. As a handicraftsman he fashioned tools and weapons, with the skilful use of which he got the mastery over all other animals. With a knowledge of the uses of fire, the art of cooking his food, and the power of fabricating materials for clothing his body, he accommodated himself to the vicissitudes of climate, and so greatly extended his habitable area on the globe. As ages rolled on he accumulated more and more of the secrets of nature, and every such addition widened the basis for further discoveries. Thus commenced the grandest revolution the organic world has ever undergone—a revolution which culminated in the transformation of a brute into civilised man. During this long transitional period mankind encountered many difficulties, perhaps the most formidable being due to the intestine struggles of inimical members of their own species. In these circumstances the cosmic processes, formerly all-powerful so long as they acted only through the constitution of the individual, were of less potency than the acquired ingenuity and aptitude of man himself. Hence local combinations for the protection of common interests became necessary, and with the rise of social organisations the safety of the individual became merged in that of the community. The recognition of the principle of the division of labour laid the foundations of subsequent nationalities, arts, and sciences. Coincident with the rise of such institutions sprang up the germs of order, law, and ethics. The progress of humanity on these novel lines was slow, but in the main steadily upwards. No doubt the advanced centres of the various civilisations would oscillate, as they still do, from one region to another, according as some new discovery gave a preponderance of skill to one race over its opponents. Thus the civilised world of modern times came to be fashioned, the outcome of which has been the creation of a special code of social and moral laws for the protection and guidance of humanity. Obedience to its behests is virtue, and this, to use the recent words of a profound thinker, "involves a course of conduct which in all respects is opposed to that which leads to success in the cosmic struggle for existence. In place of ruthless self-assertion it demands self-restraint; in place of thrusting aside or treading down all competitors, it requires that the individual shall not merely respect but shall help his fellows; its influence is directed, not so much to the survival of the fittest, as to the fitting of as many as possible to survive. It repudiates the gladiatorial theory of existence. It demands that each man who enters into the enjoyment of the advantages of a polity shall be mindful of his debt to those who have laboriously constructed it, and shall take heed that no act of his weakens the fabric in which he has been permitted to live. Laws and moral precepts are directed to the end of curbing the cosmic process and reminding the individual of his duty to the community, to the protection and influence of which he owes, if not existence itself, at least the life of something better than a brutal savage." (Huxley, on "Evolution and Ethics," p. 33.)

These humble remarks will convey to your mind some idea of the scientific interest and profound human sympathies evoked by the far-reaching problems which fall to be discussed in this section. Contrasting the present state of anthropological science with its position some thirty or forty years ago, we can only marvel at the thoroughness of the change that has taken place in favour of its doctrines. Now man's immense antiquity is accepted by a vast majority of the most thoughtful men, and his place in nature, as a derivative animal at the head of the great chain of life, appeals for elucidation to all sciences and to all legitimate methods of research. But among the joyful peans of this triumphal march, we still hear some discordant notes—notes, however, which seem to me to die with their echoes, and to have as little effect on scientific progress as the whistling of an idle wind. For my own part I cannot believe that a science which seeks in the spirit of truth to trace the mysteries of human life and civilisation to their primary rootlets, a science which aims at purging our beliefs of superstitious figments generated

in days when scientific methods were too feeble to expose the errors on which they were founded, a science which reminds us in a thousand ways that success in life depends on a correct knowledge of the cosmic forces around us, can be opposed to the highest and most durable interests of humanity.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

The Thieving of Assyrian Antiquities.

I HAD hoped that the British Museum slander case, which was decided a few weeks ago at the High Court of Justice, in regard to the calumnies which were circulated against me, would have silenced then and for ever my would-be detractors; but the review of the trial which appeared in the impression of NATURE of the 10th ult. indicates that misrepresentations are still rife, though an English Court of Justice has already sifted the matter, and gave its verdict in my favour.

2. I must answer your allegations one by one; and I ask you on public grounds to be so good as to insert my reply in the next issue of NATURE.

3. In the first place, you say "We have not referred to the case earlier, as we had hoped that some action in the public interest would have been taken by the trustees of the British Museum, which would have carried the matter a stage further. For this action however we have waited in vain."

4. The above remark plainly shows that you are not aware that I have been appealing for some time past to the trustees for a Court of Inquiry into the alleged robbery of public property, but the British Museum executive authorities persisted in refusing it. If you refer to the fourth day's trial, reported meagrely in the daily journals, you will see that I was the one who felt aggrieved that the alleged robbery of antiquities was not inquired into. The Judge was most explicit on this point, and remarked that in consequence of my representations having been ignored by the British Museum authorities I was justified in bringing my case before a Law Court.

5. Then you say, "From the evidence elicited at the trial it appeared that soon after Mr. Rassam began to dig in Babylonia, collections of tablets found their way into the London market, and these were bought by the British Museum for considerable sums of money." (*Times*, July 1.)

6. Here you are adverting to a vague evidence which was not established in Court; and if I had been called upon to controvert it, I could have shown then and there the fallacy of it, seeing that the British Museum acquired by purchase, through the late Mr. George Smith, Babylonian antiquities five years before I commenced work in Southern Mesopotamia. As a matter of fact, such antiquities have been obtainable from Armenian and Jewish dealers long before the trustees of the British Museum ever thought of conducting researches in those parts. Even I, myself, purchased a collection of tablets at Baghdad for the British Museum in 1877, long before I commenced work there, and that was by instruction from the Museum authorities.

7. Further on you state, "Now as no other excavations were being carried on except by the British Government, and as the internal evidence of the tablets indicated that those which they received from Mr. Rassam as the result of his works and those which they purchased had the same origin, it was natural that the public department should begin to grow uneasy. And this feeling became stronger when it was found that the tablets purchased were of much greater value archaeologically and historically than those which arrived at the British Museum from Mr. Rassam."

8. The whole of the above assertions are contrary to known facts and the evidence which was adduced before the Court. Excavations by the Arabs have been carried on in Babylonia from time immemorial, and as the land belongs to subjects of the Sultan, and not to the British Government, I do not know by what right you think that the British Museum can prevent others from digging and from selling what they can find to whomsoever they choose.

9. As for the "public department" becoming uneasy, it is

difficult to understand when and how such an uneasiness began and what caused it. I was always on intimate and friendly terms, as our correspondence shows, with the late Dr. Birch, the head of the Department of Assyrian and Babylonian Antiquities, until he died in 1885, or three years after my explorations ceased; and I was also in constant communication with the then Principal Librarian, Mr. Bond, until he resigned in 1888, or six years after the stoppage of the British Museum works in Babylonia; and neither he nor Dr. Birch ever made any complaint to me touching the alleged robbery of public property, though I was the only person who could have taken cognisance of the matter.

10. Then you go on to assert that the feeling of uneasiness became stronger when it was found that the tablets purchased were of much greater value, archaeologically and historically, than those sent by me. I am certainly surprised at this remark, seeing that no public inquiry ever took place regarding the value of my discoveries.

11. Then you go on to say, "Speaking broadly, it seems from the evidence that Mr. Rassam sent home 134,000 pieces of inscribed clay from Babylonia, and of these more than 125,000 are what Sir Henry Rawlinson, Mr. Maunde Thompson, and Dr. Wallis Budge style 'rubbish.' (*Standard*, June 30, *Times*, July 3.) This represented the direct return for the outlay. What did go wrong we cannot say, but the outsider will certainly think that something did go wrong in this matter."

12. Here again you are asserting what is contrary to facts, as it is known all over Europe that I am the discoverer of Sippara or Sefhervaim, and many temples and palaces in Assyria and Babylonia, from where I sent to the British Museum many valuable collections; and the 134,000 fragments were part and parcel of them. You seem to have overlooked the evidence of one of the best Assyrian scholars who is the senior assistant in the department of Babylonian and Assyrian antiquities at the British Museum, as to the value of the fragments.

13. In regard to Sir Henry Rawlinson's saying that the fragments belonging to a certain collection being "rubbish," it is certainly most startling. As you do not say where this information was obtained from, I take it for granted that it was supplied from the British Museum. Sir Henry Rawlinson would have been the very first man to condemn me if I had allowed any of the fragments to be thrown away, seeing that a mere particle might fit a broken tablet and complete an important text.

14. Further on you state that "The information which he gathered on all these points he sent home to the British Museum in the form of reports, one of the results of which was the dismissal of the native agent. On two subsequent occasions Dr. Budge visited Assyria and Babylonia, and carried on excavations for the trustees, and he acquired some thousands of tablets."

15. It is very extraordinary that the official report you quote above was withheld by the British Museum authorities from being produced in Court as a privileged document, because it contained matters which would be prejudicial to the public service, and yet a part of its contents is now revealed in NATURE.

16. In continuation of the above remarks you go on to say, "It will easily be guessed that from first to last a very considerable sum of public money, amounting to tens of thousands of pounds, has thus been spent upon excavations in Assyria and Babylonia, and the question naturally arises, Has this money been spent judiciously, and has the nation obtained what it had a right to expect in return for its money?"

17. I have no hesitation, in answer to the above remark, to say that my greatest desire is that the public should insist upon an open Court of Inquiry into the manner the British Museum executive authorities have carried on lately their Assyrian and Babylonian archaeological researches, and find out whether the enormous amount was spent "judiciously" by the different agents they have employed.

18. You further say, "Sales at auctions have revealed the fact that sundry gentlemen had been able to acquire Assyrian slabs from the palaces of Assyrian kings, and as the excavations were carried on by the Government it is difficult to account for this fact. The public has a right to know how property of this nature came into private hands, and the question must be asked until it is satisfactorily answered. The matter cannot be allowed to rest where it is."

19. I do not know what you mean by "Assyrian slabs" having been acquired by purchase, as I know of no such sale

having taken place in England or elsewhere. I am the only explorer, after M. Botta and Sir Henry Layard, who discovered "Assyrian slabs," or bas-reliefs, but that was thirty-eight years ago; and as there have been no sculptured slabs discovered in Babylonia, it is difficult to know what is to be understood by such an assertion. I do certainly agree with you that the matter ought not to be allowed to rest, but that the public should insist upon a thorough examination into the matter.

20. In conclusion, I must touch upon one more point, which appeared near the end of the criticism under discussion, about the duty of public servants to their superiors. You say, "With the terror of the decision in this case before them, all members of the public service will be in duty bound to consider whether they are able to afford the expenses of an action at law, and the enormous costs which follow in its train, before they report unpleasant truths to their superiors."

21. It will be indeed a sad day for an old public servant, who has spent the best part of his life in the service of his adopted country, and held with undiminished confidence important appointments of trust under the Crown, to be debarred from obtaining justice elsewhere when it is denied him by the department under which he served with honour, credit, and success for many a year, when his character is unjustifiably assailed.

6 Gloucester Walk, Kensington, W. H. RASSAM.

[WE are much surprised that Mr. Rassam has taken our article as personal to himself, as we dealt with the thefts in question only from a public point of view, and they might have happened, we suppose, if Mr. Rassam had never existed.

We make the following comments on some of his paragraphs:—

Para. 4. We were not aware that Mr. Rassam had been appealing for a Court of Inquiry. There is no doubt that cause has been shown for a Treasury inquiry in the interests of the public and future explorers, and we hope it will be pressed for.

Para. 6. It was no part of our duty to sift the evidence. The point is that evidence was given (see *Pall Mall Gazette*, June 30). Did not the British Museum accountant go into the witness-box to state the amounts paid for tablets? and was not the evidence dispensed with because Dr. Budge's "vague" evidence was accepted as sufficient?

Paras. 7 and 8. We referred to the evidence given in Court. What else could we do? It was not disproved in Court, or we should have said so.

Para. 9. This is a statement with which only the Trustees of the British Museum can deal. We, of course, are bound to accept Mr. Rassam's statement as he makes it.

Para. 10. We do not quite seize the point of Mr. Rassam's remark here. The statement as to the greater value of the tablets not received from him was made by the defendant; it is not ours.

Paras. 11 and 12. We can only repeat that the public is interested in knowing that of 134,000 pieces of inscribed clay sent home from Babylonia—it really does not matter by whom, 125,000 have been termed rubbish by Sir H. Rawlinson one of the trustees, the principal librarian, and the present keeper of the collection. It was not necessary to refer to any subordinate official, or to point out the singular fact that he gave evidence contrary to that of three of his official superiors.

Para. 13. We quite agree that it is most startling to hear that, in Sir Henry Rawlinson's opinion, so much of what Mr. Rassam sent home was rubbish. We presume that Mr. Rassam was startled when Sir Henry Rawlinson's deposition was read in Court; that is the reason, perhaps, that he forgot it, as he appears to have done.

Para. 15. Dr. Budge's reports could not be revealed by us because we do not possess them, and have never seen them. All the facts stated were given in evidence to which alone we professed to refer.

Para. 17. Here we cordially agree with Mr. Rassam; as before stated, in our opinion a Treasury inquiry into the expenditure of the public funds on, and the method of carrying out, excavations in the region in question since, say, 1846, is most desirable.

Paras. 18 and 19. The article was not written by an expert, and perhaps the word "bas-relief" would have been better than "slab." But there is no doubt about what we mean. Murray's "Handbook to Dorsetshire" informs us that at Canford Hall "a gallery connected with the house by a cloister is devoted to a series of Assyrian sculptures brought from

Nineveh." These sculptures—not to call them slabs—are described as "winged lions and bulls, bas-reliefs, &c., similar to those in the British Museum." Now, if there are many such galleries in England, and the objects were obtained at a low price, the whole question of excavation at the public expense is raised.—ED. NATURE.]

Bishop's Ring.

BISHOP'S RING still continues very conspicuous about sunset more so than it was for a long time previous to last November, though not so much so as at the end of 1883. It is evident there must have been an addition of some kind to the dust of our atmosphere last November, a considerable part of which yet remains. The light-coloured dust wisps reappeared faintly after the date (April 10) of my letter (p. 582), but have now entirely disappeared again (I have not seen them since July 20); the texture of the sky in the ring being perfectly smooth lately. I have not seen any other peculiar sunset phenomena of late, though Señor Arcimis (Director of the Meteorological Institute) tells me the sunsets are very brilliant at Madrid. He says he is satisfied that Bishop's Ring did not exist in Spain before the Krakatoa eruption; and he agrees with me that it has never since entirely disappeared about sunset.

Sunderland, September 11. T. W. BACKHOUSE.

Spring and Autumn of 1893.

As the peculiarities of this season are receiving attention in your pages, the following notes of things in this part of England may perhaps interest some of your readers.

On March 25 the following flowers were in bloom:—

Adoxa.	Hazel (nearly over).
Golden Saxifrage.	Larch.
Speedwell.	Celandine.
Wood Sorrel.	Cuckoo flower.
Snowdrop.	Star-wort.
Crocus (nearly over).	Broom.
Grape-hyacinth.	Peach.
Wood anemone.	Gooseberry.
Primrose.	Currant.
Violet (the white, nearly over).	Willow (nearly over).
	Cherry.

At the same date, the elm had been in flower for weeks; the ash was in full bud; one or two sycamores in leaf; chestnuts in early leaf, and showing flower buds. The hellebore had been out long ago; hyacinths were beginning to flower; a few pear buds had burst; the elms and hawthorns had shown green for several days. Butterflies had been seen for several days.

The early heat did not, I suspect, suit all our spring flowers. I saw scarcely anything this year of two of our wall-plants which are usually abundant—the *Draba verna* and the *Saxifraga tridactylites*.

On March 27 bluebells were found in flower.

The state of things now on September 12 is as curious.

The wild roses (both *Rosa canina* and *arvensis*) are again flowering in the hedges—in some cases in great profusion; there are now, or a few days ago have been, in blossom the following:

Apple.	Glastonbury thorn.
Pear.	Kerria.
Holly.	Wild strawberry.
Berberis Darwini.	Dog violets.

Rhododendron.

Some of the autumn flowers appear to me to have suffered from the heat and, perhaps, the drought. I have not seen the *Spiranthes autumnalis*, usually quite abundant here; and the *Colchicum autumnale* has been far less abundant than usual. Some garden bulbs have probably suffered in the same way.

I may add that wasps have been in extraordinary numbers; and that we have found two or three of the nests of the tree-wasp similar to the one depicted in your paper on the 7th inst.

Failand, near Bristol, September 12. EDW. FAY.

NOTES.

MR. W. SAVILLE-KENT, at present engaged as Commissioner of Fisheries to the Government of Western Australia, has shipped to London a large collection of the

stony corals, *Madreporaria*, peculiar to the Western Australian coast line. These specimens, added to the extensive series indigenous to the northern and eastern districts of Australia recently contributed by him to the National Natural History Museum, and which were amassed in association with the explorations that formed the basis of his recently published work on the Australian Great Barrier Reef, will constitute the most complete collection of Australasian *Madreporaria* that has yet been brought together. This latest Western Australian consignment includes several specimens of abnormally large dimensions that should make an interesting display in the Natural History galleries. Mr. Saville-Kent anticipates completing his Australian engagements, and returning permanently to England about twelve months from the present date.

THE French *Journal Officiel* has just published a decree for the organisation of the proposed Paris Universal Exhibition of 1900. M. Alfred Picard, who, it will be remembered, filled the office of *Rapporteur Général* for the 1889 Exhibition, has been appointed Commissary-General. He will be assisted by a consultative committee of 100 members.

THE next meeting of the Japan Society will be held at 20 Hanover-square, on Wednesday, September 27, when Prof. John Milne will read "A Short Account of Volcanic and Earthquake Phenomena of Japan." The paper will be illustrated by photographic lantern slides, and will be followed by an exhibition of photographic slides illustrating life, customs, and scenery of Japan. The meeting would under ordinary circumstances have taken place in October; it has, however, to be held in September in consequence of Prof. Milne having to return to Japan on September 29.

THE Congress of the Photographic Society of Great Britain and affiliated societies for the present year will take place on October 10, 11, and 12. The opening meeting, at which the President's annual address will be delivered, is to be held at the gallery of the Royal Society of Painters in Water Colours. The other meetings will be held in the theatre of the Society of Arts.

WE hear that Mr. Max Muspratt has just gained the diploma of Chemistry awarded annually by the Swiss Government to a limited number of students for exceptional proficiency in the scientific and technical knowledge of chemistry. Mr. Muspratt, who went to Zurich direct from Clifton College, is the first Englishman to gain this distinction.

WE learn from the *Lancet* that an interesting experiment is to be made in connection with next year's meeting of the Norwegian Medical Congress. At the meeting recently held in Christiania it was decided that next year members shall assemble on board a yacht, which will cruise during their deliberations. Weather and other things being favourable, the experiment should prove a very admirable mode of combining business with pleasure.

THE *Ceylon Observer* for August 18 has a moderately worded editorial urging the establishment of a zoological garden in Colombo, especially for the purpose of giving the many visitors who pass through Colombo an opportunity of seeing for themselves how rich and varied is the fauna of Ceylon. The *Observer* is confident that the fees paid for viewing the collection would suffice to pay the whole, or nearly the whole, of the expenses which would be incurred.

ACCORDING to the *Pharmaceutical Journal*, the Kew Herbarium has recently been enriched by the addition of forty-six dried specimens of ferns collected in Sarawak, including six new species; two hundred species of plants collected by Sintenis in Kastamuni (the ancient Paphlagonia), Asia Minor; twelve hundred dried plants collected in Kashmir; one hundred and

thirty-nine specimens of Mexican plants; and one hundred and twenty-nine of the Greenland and Iceland flora.

AN artificial horizon invented and recently patented by Mr. W. P. Shadbolt, possesses several advantages over other forms. The surface of the mercury is never exposed to the air, hence it does not easily become contaminated, and is also protected from wind. Further, loss of mercury is entirely prevented, thus dispensing with the necessity for taking any extra supply. The instrument permits very small altitudes to be observed, and after having it under trial for more than a year in tropical Africa, under circumstances entailing a good deal of rough usage, Mr. Shadbolt reports that its behaviour was most satisfactory.

THE general report of the operations of the survey of India during the survey year ending September 30, 1892, has been issued. From it we learn that the area surveyed in detail during the year amounts to 80,101 square miles. In addition to this, transverse operations have been carried on over an area of 5921 miles. By the measurement of six arcs of longitude the original scheme of differential longitude determination within India proper has been completed. Colonel Waterhouse reports upon a method he has been using to prepare a very sensitive orthochromatic collodion-bromide emulsion. Plates coated with this new emulsion require to be used in a moist state in order to obtain maximum sensitiveness and clear negatives. But wet-plates are inconvenient, and if some means could be found of preparing equally sensitive dry-plates, the process would possess considerable advantages.

WE learn from the *Botanical Gazette* that forty professed botanists are now engaged in botanical research at the fifty-four Agricultural Experiment Stations in the United States. Of this number ten are also working in entomology, three in horticulture, one in arboriculture, and one in meteorology.

NO. 2 of the *Illustrated Archaeologist* has just reached us. It is, if possible, more tastefully got up than No. 1, and archaeologists will indeed be hard to please if they cannot find something to interest them among the eight items which go to make up the present number. Among the articles we notice "Stonehenge," by Edgar Barclay; "Notes on some of the Sculptured Tombstones of Argyllshire," by R. C. Graham; and "The Roman City of Silchester," by H. W. Young; while Prof. A. C. Haddon writes on "Wood-Carving in the Trobriands." The magazine is published by Mr. C. J. Clark, 4 Lincoln's-inn-fields.

PROF. JOHN MILNE, F.R.S., has brought out, through Messrs. Crosby Lockwood and Son, a little work of which he is the compiler, and which should be very serviceable to the class for which it is intended, viz. students and others interested in mining matters. The title of the book is "The Miner's Handbook," and is a reprint, with corrections and additions, of a compilation, the first part of which was printed at Tokio some fourteen years ago. The little volume is of handy size, has rounded corners, making it suitable for carrying in the pocket, and, with the exception of the title-page, preface, and contents, was printed (and printed well) in Japan.

WE have received from the Australian Museum, Sydney, Part III. (Gasteropoda) of the "Catalogue of the Marine Shells of Australia and Tasmania," by J. Brazier. The author, in a prefatory note, says, "The present part contains only the genus *Murex*. The remaining genera are not included, from causes beyond my control."

MR. D. FORBES, 29, Victoria-street, Bristol, has sent us the catalogue of a very interesting exhibit at the Bristol Industrial and Fine Art Exhibition. It is that of the collection formed by Mr. F. Mockler, of portraits, diplomas, freedoms, grants, presentations, addresses, visiting-books, correspondence, pamphlets,

printed works, manuscripts, &c., once the property of the discoverer of Vaccination. The catalogue is prefaced by a brief memoir of Dr. Jenner.

THE September number of the *Board of Trade Journal* contains an interesting account of the development of the World's Telephones, by M. Daniel Bellet, translated from the *Economiste Français*, "A Summary of Agricultural Returns of Great Britain for 1893," "Coal Production in Japan," and many other items of interest.

WE have just received the Proceedings and Transactions of the Royal Society of Canada for the year 1892. The London agent for it is Mr. Bernard Quaritch.

A LIST of observations made in 1892 by the members of the Caradoc Field Club, Shropshire, and others, has been issued under the title, "The Caradoc Record of Bare Facts."

THE report for 1892 of the Botanical Exchange Club of the British Isles has just reached us. It is issued by Messrs. James Collins and Co., Manchester.

THE Calendar of the Durham College of Science, Newcastle-on-Tyne, for the session 1893-94 has reached us. It is published by Messrs. Andrew Reid, Sons, and Co., London, and Newcastle-on-Tyne.

MESSRS. F. AND E. GIBBONS, Liverpool, have issued prospectuses of Day Classes in Arts and Science, and of the Evening Lectures, and of the Department of Engineering, in connection with the University College, Liverpool, for the session 1893-94. They contain all the preliminary information which intending students are likely to require.

THE Calendar for 1893-94 of the Glasgow and West of Scotland Technical College has just been published by Mr. Robert Anderson, Glasgow.

DR. W. H. PEARSE has sent us a copy of the address delivered by him as president of the Plymouth Institution and Devon and Cornwall Natural History Society, at the opening meeting of the session 1892-93. It deals with the subject of our present knowledge of biological science.

WE have received a printed circular signed "A Free Lance," condemning a recent action of ours in refusing to print a letter from the author on the subject of the "Publication of Physical Papers" unless, in accordance with the rule to which attention is drawn in every number of NATURE, he divulged his name.

We fail to see any adequate reason for violating our rule in the favour of "A Free Lance" more than in the case of any other of our correspondents.

THE products of the sublimation of arsenic form the subject of an important communication to the latest number of the *Zeitschrift für Anorganische Chemie*, by Dr. Retgers. The various allotropic modifications of the element have been subjected to a searching investigation, and further interesting information has been acquired concerning the little known solid hydride of arsenic AsH , and the suboxide As_2O , whose existence has hitherto been considered doubtful. It is first shown that there is no amorphous modification of arsenic, the deposit of so called "black amorphous arsenic," obtained during the sublimation of the element in a current of hydrogen and in a number of high temperature decompositions of arsenic compounds, is found to be microcrystalline and to exhibit distinct evidence that it consists of the ordinary regular variety. There are consequently only two well-defined allotropic modifications of arsenic: (1) the stable form which crystallises in the hexagonal system, is silver-white in appearance, specifically heavy, and requires a comparatively high temperature for volatilisation; and (2) the specifically lighter and more volatile modification which crystallises in regular octahedrons, and exhibits a black surface. When arsenic is heated to the point of sublimation in

a current of indifferent gas, the first variety condenses nearest the heated element, while the second variety, owing to its low temperature of volatilisation, is deposited more remote from the portion of the tube which is heated by the flame. These two forms of arsenic correspond completely with the two modifications of phosphorus, the regular black variety with the regular yellow modification of phosphorus, and the silver-white hexagonal form with the hexagonal red phosphorus. Dr. Retgers adduces some evidence also in support of the view that there is a third crystalline modification of arsenic, of which the crystals belong to the monoclinic system. The fact is also recorded that elementary arsenic, of whatever modification, is invariably opaque even when in the finest state of division; former observations of yellow and brown transparent arsenic are shown to have been due to compounds having been mistaken for the element. The brown spots which are deposited along with arsenic when the flame from a Marsh's apparatus is allowed to impinge upon cold porcelain, or which form in the Marsh sublimation tube, do not consist of the element in thin layers, but of the brown solid hydride AsH , which is produced by the partial dissociation of the gaseous trihydride AsH_3 . When the last-named substance is heated during its passage along the sublimation tube, the arsenic mirror consists of both modifications of the element, the silver-white hexagonal form nearest the flame, and the black regular variety further removed. The brown deposit of solid hydride AsH is always found furthest removed of all, and thus forms, as it were, the tail of the mirror. Dr. Retgers finds that boiling xylene readily dissolves this solid hydride, thus affording an excellent mode of distinguishing it from elementary arsenic, which is totally insoluble in xylene.

THE solid hydride of arsenic was isolated some years ago by Janowsky, who obtained it as a brown velvet-like substance by decomposing potassium or sodium arsenide with water. Ogier has subsequently shown that it may likewise be obtained by the action of the silent electrical discharge upon the gaseous trihydride. The existence of the suboxide of arsenic has hitherto been so doubtful that the compilers of the new edition of "Watt's Dictionary of Chemistry" feel justified in stating that "no definite proof of the existence of an oxide with less oxygen than As_2O_3 has been given." Dr. Retgers, however, adduces weighty evidence in favour of the supposition. As explained above, when arsenic is sublimed in a current of inert gas (carbon dioxide, for instance), a deposit consisting of the two modifications of the element, white and black, is obtained. The moment, however, that a little oxygen or air is allowed to mix with the carbon dioxide a brown annulus commences to form between the white and the black elementary deposits. This brown sublimate is not crystalline, and is transparent, the thin films proving to be quite isotropic in polarised light. In order to produce a broad deposit of this brown substance it is advisable to employ a wide tube, and to stop the experiment as soon as the annulus is formed, as further heating soon decomposes it again. From this and further evidence adduced by Dr. Retgers, there can be no longer any reasonable doubt that a lower oxide of arsenic, probably As_2O , is capable of existence.

THE additions to the Zoological Society's Gardens during the past week include a Campbell's Monkey (*Cercopithecus campbelli*, ♀) from West Africa, presented by Miss Jane Richards; a Pig-tailed Monkey (*Macacus nemestrinus*, ♂) from Java, presented by Miss Llewellyn; an Azara's Capuchin (*Cebus azarae*, ♀) from Paraguay, presented by Miss Hairby; two Common Marmosets (*Leopoldus jacchus*) from South-East Brazil, presented by Mr. E. Lake; two Lions (*Felis leo*, ♂ ♀) from East Africa, presented by H.H. the Sultan of Zanzibar; two Egyptian Jerboas (*Dipus aegyptius*) from Egypt, presented by Miss B. Dell; two Egyptian Jerboas (*Dipus aegyptius*) from

Egypt, presented by Mr. M. W. Edgley; one Egyptian Jerboa (*Dipus aegyptius*) from Egypt, presented by Mr. W. R. Clark; a Golden Eagle (*Aquila chrysetus*) from Scotland, presented by Mr. Bryan Cookson; a — Buzzard (*Buteo* —) from West Africa, presented by Mr. Rice; three Tench (*Tinca vulgaris*) from British Fresh Waters, presented by Mr. Arthur E. Rumsey; two Collared Fruit Bats (*Cynonycteris collaris*), a Wapiti Deer (*Cervus canadensis*, ♂), a Japanese Deer (*Cervus sika*, ♀) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

DOUBLE STAR MEASURES.—Nos. 3185-86 of the *Astronomischen Nachrichten* contains the micrometrical measures of double stars made by Mr. Tarrant during the years 1889-92. This series is a continuation of that published in a preceding number (2991) of the same journal. The same instrument has been employed as formerly, but its position has been changed, it now being 510 feet above the sea level. Stars with considerable southern declination can thus be much more accurately measured. The objects are arranged in the following order:—Dorpat Catalogue, Pulkova Catalogue, Burnham, and Miscellaneous.

PUBBLICAZIONI DELLA SPECOLA VATICANA.—In the third volume of this publication there are several contributions of interest and importance to which we can here briefly refer. In the Astronomical Section, M. P. G. Lais gives an account of the measurements of the position of Nova Aurigæ (with a photograph), and also a few words on the comets Swift, Holmes, and Brooks. M. P. F. Denza, in addition to a communication on the total eclipse of the moon that occurred on November 4, 1892, gives a summation of the observations made of the shooting stars of August in that year, and of the shooting stars of November in the same year, and also of solar spots, magnetic disturbances, and auroræ. In the Astro-Photographic Section, M. P. G. Lais and F. Mannucci give an account of the work done for the international chart and catalogue of the heavens; twenty-six photographs for the chart, and 115 for the catalogue were taken, while 154 other photographs, including groups of stars, nebulae, comets, &c., were obtained. These communications are accompanied by some fine photographs, which include the Præsepe group, Nebula of Orion, and some of the sun. M. P. F. Denza communicates most of the articles in the Magnetic Section, while the Meteorological Section contains many important communications, with several diagrams, among which we must mention that on the classification of clouds, by M. F. Mannucci, which is illustrated by a beautiful series of photographs showing the various forms which they assume.

COMET FINLAY AND THE PRÆSEPE.—The ephemeris of Finlay's comet showed that a passage through the star-group Præsepe would take place about the beginning of October. In *Astronomischen Nachrichten*, No. 3187, Prof. A. Berberich gives a comparison of the ephemeris with the star-places in Yarnall's catalogue the measures of C. Wolf and Winnecke giving the following table of conjunctions. (γ. Pr. = number in Yarnall's catalogue):—

γ. Pr.	Conj in R.A. M. T. Paris. h	Comet-Star Δ δ
5	2 Oct. 20'9	— 0'2
9	3 " 3'8	— 2'3
16	3 " 15'1	— 1'4
37	4 " 6'0	+ 0'3
59	4 " 21'1	— 0'6
69	5 " 2'2	— 1'0
74	5 " 3'0	+ 0'2
89	5 " 7'5	+ 1'0
134	6 " 8'8	— 0'0
148	7 " 12'7	— 1'4
150	7 " 17'0	+ 1'9

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 11.—M. Lœvy in the chair.—Treatment of vines infested by *Phylloxera* with peat moss impregnated with schist, by M. F. de Mély. The results of

the treatment proposed last year have been controlled by the Inspector-General of Agriculture and by the Inspector-General of the Compagnie de Lyon. As a consequence of their visit, the Minister of Agriculture has sent Dr. Colas, of Lyon, to organise further applications of the method. A vine already attacked by *Phylloxera* has been under treatment since June, and although some of the insects have survived, the vine has not turned yellow. The portion of the vineyard treated by this method for two seasons has retained its rootlets in a perfect state, and those vines which were treated with the maximum dose—viz. 2 kgr. of the mixture, containing 200 grs. of pure schist, show no trace of *Phylloxera*.—Magnetic observations recently made in Russia by M. Venukoff. Observations at about a hundred stations comprised between 45° 11' and 36° 42' of latitude, and 65° 47' and 82° 17' of longitude east of Greenwich prove that the isogonal lines inserted in Berghaus's *Physikalischer Atlas* are not exact for Central Asia; in particular, the degrees of declination accepted are too large. Local variations of the magnetic elements in European Russia have recently been investigated, and some very large disturbances have been discovered. In the province of Grodno the magnetic declination was found to change by 10° in a distance of 21 km., and in the neighbourhood of Belgorod the deviation mounted up to 180° in a space of a few tens of square km. This implies the presence of a small and perfectly local magnetic pole. It must be remembered that in the Neva delta the fortress of St. Peter and Paul is known to deflect the magnetic needle by 10°.—Presence of a ferment analogous to emulsine in mushrooms, and particularly in parasitic mushrooms of trees or those growing on wood, by M. Em. Bourquelot. It is proved that several mushrooms, and especially those developing on living or dead wood, contain a soluble ferment possessing the property of doubling various glucosides, such as amygdaline, salicine, and coniferine. It is not possible to say that this ferment is identical with the emulsine of the almonds, but it acts in the same manner and upon the same substances. This ferment was found in two ways. In one, the fresh mushroom was placed in a saturated atmosphere of ether or chloroform vapour, which produces an abundant exudation of liquid holding in solution a large portion of the principles contained in the cellular juice. This liquid was placed for 24 or 48 hours in direct contact with a solution of a glucoside; or an aqueous solution was formed by precipitation with alcohol, and treated in the same manner. In the second process, the mushroom was triturated with sand and transformed into a paste; this paste was treated with distilled water and filtered off, the liquid being used as before. One specimen, picked from an elder branch, gave a liquid which completely converted a dose of coniferine into grape-sugar in the course of three days. The ferment is limited to fungi living on wood, enabling them to assimilate the glucosides contained in it.—On a method of determining the density of gases for industrial purposes, by M. Maurice Meslans.

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